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NOTICE.

“That, although the judges have fairly and impartially decided in favour of this Essay, neither the giver of the Prize nor the Ladies’ Sanitary Association are to be considered as accepting the principles advocated, or the conclusions arrived at, by the Author.”

“Telle est la vaccine, si douce dans sa marche, si précieuse dans ses conséquences, que jamais peut-être il n’a été donné à l’homme de jouir d’un si grand bienfait à un moindre prix.”—BOUSQUET.

ON

VACCINATION :

ITS VALUE AND ALLEGED DANGERS.

A PRIZE ESSAY.

*[Being the Essay to which the Prize of One Hundred Pounds was
awarded by the Adjudicators appointed by the Committee of the
Ladies' Sanitary Association.]*

BY

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P R E F A C E.

THE title-page of this work explains sufficiently the circumstances under which it was undertaken.

I can honestly say that I have striven to the best of my ability to exercise a fair and impartial judgment upon all controverted points, and have spared myself no pains to arrive at the truth.

My principal difficulty has been to render a work upon a medical subject, involving some obscure problems in pathology, comprehensible by lay readers. Still, it was necessary to make the attempt, since the interest of the subject extends outside the circle of the profession. The division of the subject I have adopted is that indicated by the terms of the competition, which left me no choice.

The only addition made to the original manuscript consists in some extension of the remarks on "animal vaccination," the practice of which I had

an opportunity of studying when in Paris last year. I am indebted to M. Depaul and M. Chambon for the kind manner in which they promoted the object of my visit.

In place of an Index I have thought it more advantageous to the reader to prefix a copious Table of Contents, which, being of the nature of a skeleton, shows readily the line and method of argument which I have adopted.

E. B.

7, COMPTON TERRACE, ISLINGTON, N.;

April, 1868.

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INTRODUCTION.

SIXTY-NINE years have elapsed since the announcement was made to the world of the protective power of vaccination against a disease which, during the previous hundred years, is calculated to have destroyed 45,000,000 of the people of Europe. So important a discovery as that of Dr. Jenner was not likely to have passed unheeded by his medical contemporaries. They hastened to test its value. One after another of those whose opinion carried weight with the public, both at home and abroad, in quick succession declared themselves in favour of the novel proceeding. In less than two years from the publication of Dr. Jenner's book, the practice of vaccination, which had meanwhile made progress in this country, was introduced into Hanover, and from thence became extended over the whole of the European continent. It was early introduced into America, and into our own Indian possessions in 1803. As facts accumulated, and confidence became established, national governments became impressed with the benefits likely to accrue from the extensive diffusion of vaccination among their populations, and in different states decrees of varying stringency were promulgated, with a view to bring about the general adoption of the practice. I am not aware of any instance in which national confidence thus manifested has been withdrawn.

There appears, then, to be an universal assent to the prophylactic virtue of the practice of vaccination—universal national assent; and with it, for the most part, individual

conformity. It would be well for us if I could add upon the evidence of facts, that individual conformity demonstrates in all instances individual conviction. It is an easy thing to conform, to take for granted the scientific truth of the basis on which such a popular practice as vaccination rests; but conviction demands previous thought, and this is too troublesome a matter for the mass of persons. It is true that the rumour of an epidemic of smallpox never fails to bring an abundance of applicants of all classes for protection. But, after all, this is but an indication of alarm, and that, up to the present time, the fact that such protection is given by vaccination is not generally questioned. Even in ordinary seasons the better educated classes of the community rarely neglect to obtain the early vaccination of their own children: it is done, as a good many other things are done, as a matter of course and routine; but we do not see an equally large proportion seeking, what is nearly as important, a renewal of their own protection on attaining early adult life. And when we pass to a lower social grade, and inquire to what extent primary vaccination is practised among the poor, among those for whom the state provides the opportunity for gratuitous vaccination, we are brought face to face with a condition of affairs by no means creditable to the convictions of those who are entrusted with its diffusion in their respective neighbourhoods. Dr. Seaton tells us, in his inquiry into the state of vaccination in Yorkshire, Herefordshire, and Wales,* that in 1863 he found many districts where no public vaccination whatever had been performed for periods varying from one to three years, and that there was not one large town or town of moderate size that he found in anything like a state of efficient protection against smallpox. Dr. Seaton, Dr. Buchanan, and Dr. Sanderson, all speak of

* Sixth Report of the Medical Officer of the Privy Council.

the large number of unvaccinated children to be found in the schools all over the country. Thus, out of over 23,000 children in workhouse and union schools, Dr. Seaton found 12 per cent. (in infant schools 14 per cent.) to be unvaccinated; and about a fourth of these were scarred with smallpox. The neglect of vaccination, as might be anticipated, is more flagrant in some places than in others. Thus, in one district examined by Dr. Sanderson, *one half* of the children in the schools were unvaccinated, and in others the proportion of unprotected has been found to vary from 40 to 20 per cent. Even in the workhouse schools, where the children, one would think, were most favorably situated for receiving protection, this neglect is largely observable. In Dewsbury workhouse 38 per cent. of the children were found to be unvaccinated, in Chester 35 per cent., in York workhouse 38 out of 109, in Leeds workhouse 32 per cent., in Selby workhouse 30 per cent., and so on. Infant schools generally, in all parts of the country, were found worse vaccinated than others, the unprotected children in some districts forming a proportion of from 20 to nearly 50 per cent. of the whole number. In London, indeed, the vaccination of the poor is on the whole more completely carried out. On an average Dr. Buchanan found only about 5 per cent. unprotected in the schools, and 6 per cent. in the infant schools; but more than a third of the unvaccinated children were scarred with smallpox. I have myself examined some thousands of school children in London, and upon about one tenth of them I could discover no mark whatever to indicate that they had undergone vaccination.

Now, although in the case of union schools and workhouses the blame of all this clearly lies with the managers, who, if they had a due sense of the importance of vaccination, might insist upon all the children under their care being protected, it is clear that, in other cases, much of the fault

rests upon the parents of the poor children. But, even in this case, no one conversant with the details of public vaccination in England can have any doubt that the pains taken to ensure its completeness, and the amount of encouragement accorded to its administrators, are very far from what the necessities of the case demand.

I quite agree with Dr. Seaton,* that, amongst the poor, indolence and indifference are mainly concerned in the neglect of vaccination, and that, taken altogether, there is no actual rooted objection to vaccination in the minds of the people. Still in some minds objections do exist. Unhappily it is not the poor only who entertain them, although with these a deficient education naturally impairs the exercise of reason and judgment. It remains for us to inquire upon what basis the opposition to vaccination, active or passive, as the case may be, appears to rest, and to what it may be believed to be due.

Now, I think that a successful attempt may be made *to classify those who are antagonistic to the practice of vaccination*. First, then, to begin with the smallest class, I believe that there are a few persons yet living whose objection to vaccination is traditionary: their fathers and grandfathers objected before them, they never inquired into the subject for themselves, and only repeat what has been handed down to them from their childhood. Secondly, there is a small class of persons whose minds are so contorted that the assent of a majority is certain to place them in the ranks of the minority. Such persons profess to think upon all subjects for themselves, but, nevertheless, somehow or other, the conclusions they arrive at are not the same as those arrived at by other people. These eccentric folks are totally unmanageable. To argue with them is to throw time and

* 'Smallpox and Vaccination.' Return made to an order of the House of Commons, April 26th, 1853, p. 21.

labour away. A third class of objectors are absolutely dishonest. They have sinister purposes of their own to serve, and, to gain their object, suppress some truths, exaggerate others, and misstate others. It is from the mouth of these persons that the cry of "danger" mostly proceeds. The real danger lies in listening to them, and unfortunately they find most of their listeners among the poor and ill-informed. But the fourth and most important class of objectors is constituted of thinking but illogical people, who, having only heard one side of the question, and being unaccustomed to sift and weigh the value of evidence, are apt to form their judgment upon insufficient grounds. Such persons may have met, within the circle of their own friends or within a limited experience, a number of instances, possibly consecutive instances, where smallpox has attacked persons who are reputed to have been vaccinated, and without further investigation they conclude that vaccination furnishes no protection against this disease. These, at least, are people who have an honest conviction—possibly, indeed, strengthened by traditionary prejudice—but who are fairly amenable to argument. They may be shown, for instance, how an extended experience overthrows the inference they have drawn from too limited data; how possibly the data themselves are open to fallacy; and how many of the dangers which they are apt to magnify in importance become dwarfed in their proportions when looked at from a different point of view to theirs.

Where to place a class of medical men—very small indeed, I should hope and I believe—of whom Dr. Hamernjk of Prague may be regarded as the representative, I really do not know. A more extraordinary expression of faith, or rather of faithlessness, than that conveyed by this gentleman in a letter to Mr. Simon,* and founded on more illogical reason-

* Papers relating to the 'History and Practice of Vaccination,' p. 127.

ing, I must say I have very rarely had occasion to meet with. But it, nevertheless, has its value, inasmuch as it gives us an insight into the probable working of other minds than his, and shows us the difficulties with which they may have contended, and by which they may ultimately have been overcome. As it will be the aim of this essay to convince honest objectors, untrained in logical processes, and who have thus adopted one or more of the arguments against vaccination put forward by Dr. Hamernjk, it will not be amiss briefly to state the grounds of his opposition to the practice of vaccination. They will be discussed either directly or indirectly in the course of the essay, so that in this place I shall make no observation respecting them, except where they originate in a misunderstanding of the statements of others. First of all, then, Dr. Hamernjk expresses surprise at Jenner advocating the use of cow-pox matter and repudiating the inoculation of the pus of smallpox, seeing that he considered both affections to be identical in nature. We can only say in reply to this that a very superficial perusal of Dr. Jenner's works would have removed from the writer's mind all occasion for wonder.* He further says that it is a great mistake to suppose that smallpox presents different characters, according as persons have been vaccinated or not; but then it is to be said, in explanation of this, that Dr. Hamernjk includes chicken-pox as well as all degrees of smallpox under one common designation. He asserts, in support of his views, that vaccinated persons may be attacked with smallpox either during the period of the development of the vaccine or a few days after its drying up, and that the two diseases may luxuriate together upon the same individual. He objects that diametrically opposed views are held by medical men of eminence as to the tendency of vaccination to favour the development of smallpox,

* See especially a letter from Dr. Marshall in "A Continuation of Facts and Observations, &c.," p. 14.

and hence as to the propriety of vaccinating young children during epidemics of this disease. Here Dr. Hamernjk confounds two things which are distinct. The controversy he alludes to is not as to the favouring or not of the development of the prevailing disease, but, the contagion being received into the system, whether the evolution of the smallpox is hastened or not by the vaccination. On the other hand, he asserts that a pretty extensively spread epidemic of smallpox often prevents the development of cow-pox. We shall see hereafter that trustworthy authors have held the very opposite opinion as the result of their observation. Then he says broadly that we learn from all well-marked smallpox epidemics that cow-pox does not protect from smallpox, even after repeated vaccination; and that confidence in vaccination was especially shaken in England by the epidemics of 1825, 1838, 1840, and 1841; and further, that it was useless in the epidemic of Paris in 1826, and in the Marseilles epidemic of 1828. He considers of no value the statement that epidemics of smallpox have been arrested and rendered milder by rapid vaccinations and revaccinations, because medical men could not possibly prove that such was the case, inasmuch as epidemics prior to vaccination varied much in their degree of severity. Finally, and this is the unkindest cut of all, he repudiates the official returns on which most reliance is placed by the advocates of vaccination, on the ground that they are likely to be constructed in a manner to render them "palatable to gentlemen high in office!" As regards the characters of smallpox, before and after vaccination, he contends that comparative mildness of the disease had been occasionally noticed prior to the use of vaccination, and he attributes the less general severity of type in the present day to the advantages obtained by a higher civilisation, and by a more general diffusion of the means of subsistence. He asserts that, in his experience, under judicious treatment, smallpox is only

fatal in about four per cent. of those attacked, forgetting, however, to add here how many of those he treated for the disease had undergone vaccination previously, and also that he includes among his cases of smallpox a disease—chicken-pox—which is hardly ever fatal at all. He attributes the less degree and frequency with which smallpox is followed by pitting of the face, in a similar manner, to the effects of remedies, and to the mildness with which smallpox sometimes runs its course independently of vaccination. In the last place, he denies the contagiousness of smallpox, holding that it spreads only in two ways, epidemically and by inoculation (the latter being often effected by simple inhalation). But this is mere playing with words. Then he objects that the doctrine of Jenner is opposed to acknowledged pathological principles—

“As if rules were not in the schools
Derived from truth, but truth from rules.”

His first argument under this head is that observation has taught us that two severe diseases cannot affect an individual at the same time; concluding (as I gather his meaning) that, as cow-pox can and does coexist with severe diseases, it cannot be of a nature to affect the whole organism. The next is, that it is a general pathological law that morbid actions which have entirely run their course can have absolutely no influence whatever upon the subsequent pathological relations of the individual, and that the scars left by vaccination can have, therefore, no more influence on the diseases which subsequently attack him than any other scars. Now here, again, Dr. Hamernjk mistakes the opinion held in the profession. No one for a moment would think of asserting that the scars of vaccination, as scars, afford protection from smallpox; they are never by any author alluded to at all, except as being outward signs and relics of a previous dis-

ease on which the protection depends. And as to the law he promulgates, Dr. Hamernjk appears to have completely forgotten how few persons who have undergone one attack of smallpox, measles, whooping-cough, or scarlet-fever ever suffer from a second invasion. His last objection is based upon the accidental occurrences of syphilis after vaccination in certain cases on record. To this subject, however, I shall recur towards the close of my essay, and shall endeavour to show to what extent syphilitic inoculation constitutes a real danger, and how far that danger is valid as an objection to vaccination.

After all, the true questions we have to decide are these, whether vaccination, as a protective measure against smallpox, has established for itself a claim to our confidence, or whether it would be to the general advantage of mankind to abandon its practice altogether; and further, whether it is an operation so harmless as to commend itself to our acceptance, or whether, on the other hand, it is so encompassed, directly or indirectly, with dangers, that the latter counterbalance the benefits it is believed to confer.

The reply, and the only reply, that can be given is an appeal to facts and the experience of nearly seventy years. Were I writing for my own profession alone, I should content myself with making this appeal, and should proceed at once to the consideration of the subject; but as it may be presumed that some at least of my readers may be less familiar with the nature of diseased processes, it becomes necessary that I should premise some remarks of a general character, and some special information respecting those diseases which will form the main topics of my observations.

The Nature of Smallpox and Vaccinia, and the Operation of the Virus of each upon the Body of Man.

Smallpox and cow-pox, or *vaccinia*, are members of a tolerably large class of maladies which affect man and the lower animals, and are known as the "exanthematous diseases," or as the "febrile eruptive diseases." To the same class belong those very familiar maladies, chicken-pox, measles, scarlet fever, &c. Whatever the original cause of each of the members of this class may have been, the only way in which we *certainly* know of their arising in any individual *now* is by direct or indirect communication from one suffering from the same affection.* This is, I believe, absolutely true of smallpox and the vaccine disease. The communication is believed to take place by the passage of a material *something* from the sick individual into the system of the healthy individual. This "something" is termed the "matter of contagion," "the *contagium*" (Simon), the "morbid poison," or perhaps, best of all, and most appropriately, the *virus* of the disease.

It would be out of place here to discuss at any length *the nature of a morbid virus*. The most interesting inquiry of the kind entered into of late years was that instituted by Dr. Lionel Beale, in his investigation of the virus or contagium of cattle plague.† Whatever it is which the sick indi-

* I make this statement broadly and in general terms. There is, however, good reason to believe that erysipelas arises commonly in another manner, and it is a common belief that typhoid fever has a similar origin in certain general unsanitary conditions. Dr. Salisbury also believes that he has seen an outbreak of measles arise from exposure to the action of low vegetable organisms developed on mouldy straw, and that he has produced the disease by their inoculation.

† Appendix to the Third Report of the Cattle Plague Commissioners.

vidual gives off capable of imparting the same disease to another, the following things may be said of it:—1st. That it is material. 2nd. That it is something not given off from the healthy, and so no part of his healthy structure or secretions. 3rd. That it is contained in the specific products of the diseased action. 4th. That probably it is something endowed with vitality, in the sense that it is capable of growth, extension, and reproduction. Dr. Beale calls this living material “germinal matter,” and in the instance of cattle plague he thinks that he has probably seen it under the microscope. 5th. That it produces its results in a formerly healthy individual by virtue of its character as a living thing. 6th. That being a *living thing*,* it is subject to the contingencies to which all other living things are subject, such as the operation of a variety of surrounding conditions promotive of the manifestations of its vitality, calculated to suspend its manifestations for a time, or to suspend them altogether. The last operation is equivalent, of course, to the death of the matter of contagion—of the virus.

In the case of smallpox and vaccinia the virus is contained in the specific secretion or contents of the pustules; and when a little of these contents is taken upon the point of a lancet, the skin punctured, and the virus then introduced into the wound, the specific disease, smallpox or vaccinia, as the case may be, is produced in the person operated upon. This is called *inoculation*.

And now, *what happens?*

Let us begin with the instance of *the inoculation of smallpox virus*. First, then, a local and subsequently a general result follows. The first effect observed is the rising of a vesicle

* Although a “living” thing, it is not necessarily an “animal” product. It may possibly partake of the nature of a vegetable production. It is not necessary to define which it is, even if the state of science would permit of this being done.

about the third or fourth day from the inoculation. That this vesicle contains the virus, multiplied locally at the spot, may be shown by the capability of inoculating another person from its contents. The vesicle has a depressed centre, and is mounted upon an inflamed base. On the fifth day it is fully developed, and by the sixth the local inflammation has greatly increased, and some stiffness in the armpit and pain are experienced. And so it goes on up to the end of the seventh or beginning of the eighth day, about which time the contents of the vesicle become purulent, and the general system exhibits symptoms of disturbance.

The first of these general symptoms are referable to a disturbance of the nervous system, "rigor" or shivering—a convulsive phenomenon, headache—a sensory disturbance, vomiting—an excito-motory phenomenon, languor, sensation of heats and chills, and offensive breath usher in fever. Sometimes the rigors are, as in casual smallpox, more severe, and put on the characters of ordinary convulsion of children or "eclampsia." On the eighth or ninth day spots of smallpox eruption, the "secondary eruption," appear in various parts of the body, and the smallpox runs its regular and ordinary course, the febrile and nervous phenomena subsiding when the eruption has *all* come out.

When smallpox is taken casually, by ordinary contagion; that is, when the virus gains access to the system in some other way than by inoculation, we observe the same series of phenomena; only the duration of the period between exposure to the contagion and the appearance of the general eruption is much longer. The difference, I imagine, is due to the time which is required for the virus to make its way through the surface with which it has come into contact, and through the walls of its vessels into the blood. In inoculation it is actually introduced into the blood by the section of the small vessels of the skin in the making of the puncture.

The first symptom (except some indefinite discomfort, perhaps, during the *period of incubation* or time elapsing between the reception of the contagion and the occurrence of the disease) is here too referable to the nervous system—rigors, convulsion, &c. &c., the fever abating when the eruption has all come out.

Next we may take the results of *the inoculation of vaccine virus* or vaccination. After some very trifling local irritation at the point of puncture, there is to be felt by the finger, on the fourth day, a little hard point, which, on the fifth day, has become slightly raised above the surface, in the form of a little vesicle with a depressed centre, and if punctured is found to contain a clear fluid. That this vesicle contains the virus, locally multiplied, may be shown by the fact that the vaccination of another individual may be performed with its contents. The vesicle continues to grow, and by the eighth or ninth day commences to be surrounded by an inflamed red circle of skin; and as this “areola,” as it is termed, becomes more perfect, so the contents of the vesicle, previously clear and limpid, become gradually opaque.

The further progress of the vaccine disease and its character depend very much upon the activity of the lymph primarily used for the vaccination. I will assume that it is sufficiently active to produce the characteristic features of the disease. Well, then, at the time that the areola has become fully established, evidence is furnished of general disturbance of the constitution. In some cases the glands in the armpit become enlarged and painful, slight feverishness occurs, the child becomes restless and cries a good deal, and sometimes the pulse becomes quickened. In other cases still further evidence of constitutional disturbance is furnished, but this is rare. Thus there may happen more distinct evidence of a disturbance of the nervous system, such as appears prior to the occurrence of the general eruption in smallpox inoculation, and some-

times also really a *secondary eruption*. Thus Dr. Drysdale* relates a case occurring under his own observation, where, the vaccine eruption having progressed normally up to the ninth day, convulsions—"eclampsia"—set in, and with it there appeared a papular eruption,† generally over the body, and the child quickly died. The secondary eruption has often been observed by vaccinators using very active lymph, either derived immediately from the cow, or but few human removes from it. Mr. Ceely says he has often seen it in young healthy children with tense, sanguine skins, in warm weather, and believes it to be a true secondary vaccine eruption. He relates a case‡ of a young lady, in which, during the desiccation of the vesicles on the arm, a secondary eruption of perfect vaccine vesicles appeared on the face and trunk, which went through their regular course, leaving characteristic scars. When vaccination was first introduced, the Rev. Mr. Holt states that he vaccinated some 300 persons, and that two of them presented a general eruption of about 100 pustules all over the body, which resembled those produced at the punctured spots, only that they were smaller. He inoculated from them successfully.§ A remarkable instance is given by M. Chonnaux-Dubisson.|| The child, three and a half years old, had four fine vaccine pustules developed upon the arm. On the ninth day the whole body was covered with vesicles, and there was active fever. The new pustules were umbilicated and very painful. Two infants were actually vaccinated from these secondary vesicles, and fine results were obtained, the disease running its course in a normal manner. The

* 'Med. Times and Gazette,' 1863, vol. i, p. 545.

† There is, however, no proof afforded that this was not an eruption of smallpox, the circumstances of the patient not being described.

‡ 'Trans. of Prov. Med. and Surg. Association,' vol. viii, p. 377.

§ 'Med. and Phys. Journal,' vol. ii, p. 402.

|| 'Rapport sur les Vaccinations en France pour l'année,' 1862, p. 26.

secondary eruption referred to was confluent like smallpox, and lasted twelve days. Instances of supernumerary vesicles—that is, of vesicles, one or two, formed at other spots than those punctured—are not very rare. Most vaccinators of experience have met with them occasionally, and have sometimes attributed them to re-inoculation by scratching, or some similar accident. I have met with them once or twice myself. In the last instance I saw, in 1866, there were four excellent vesicles upon the arm of a female child, which had appeared tardily, and two equally characteristic upon the left labium. M. Trébault, quoted by Bousquet,* has seen the vaccine disease pass quietly through all its stages, and then, when all was completed, recommence at the same spot, and pass through its stages again as at first. I think it is clear, then, from all that I have said, that the vaccine disease does, when the virus is sufficiently active, more or less operate, not only locally, but also generally throughout the system. Let me add that the vaccinia is not satisfactory or complete as a disease unless it does thus manifest *some* general operation, varying, as this must do, as it may be expected to do, with the varying constitution of the subject who undergoes it. Still, for the most part, the constitutional disturbance is very moderate in degree, and in adults vaccinated for the first time, even with active virus, does not hinder them from following their ordinary avocations.

I have only further to add that the *areola* is usually held to be a result and evidence of the constitutional operation of the virus. It is very unlikely to be due to the mere presence of the local vesicle, and to the irritation and determination of blood it occasions; because, if it were so, the introduction of other irritants into the tissue of the skin might be expected to occasion a similar phenomenon. Nicolai, for instance, introduced into the skin such irritants as tincture of cantha-

* 'Traité de Vaccine,' p. 60.

rides, pus from an abscess, mineral acids, &c.; but, although they caused more or less inflammation, nothing was ever produced at all similar to the vaccine areola. Besides, it is worthy of observation that the areola commences and develops itself at the precise period when the general constitutional disturbance produced by active virus occurs, and appears thus to form part of it.

I need not describe further the progress of the local eruption beyond the eighth or ninth day, as I shall have occasion, when comparing the action of viruses of different dates of origin from the cow, to enter upon this subject very fully. I shall therefore now content myself with requesting my reader to observe the close, very close similarity, even to dates of their occurrence, between the phenomena observed after the inoculation of smallpox and the inoculation of vaccine virus. And I will especially ask that this may be noticed, that in both the first phenomenon is a local eruption at the punctured spot, then about the eighth or ninth day a general constitutional disturbance, less marked, however, in the instance of vaccinia than in that of smallpox, yet nevertheless distinctly observable and exceptionally *well* marked.

Let us now inquire into the *modus operandi of the virus*; what it has been doing to produce these results, and how it brings them about. We have two things to explain: the production of a local eruption, and the production of a general influence, accompanied, in the instance of smallpox, with a general eruption also. That is, we see constantly in smallpox and vaccinia a local multiplication of virus, and constantly in smallpox, exceptionally in vaccinia, a systemic multiplication of virus. *E nihilo, nihil fit*. If virus is thus multiplied—manufactured, so to speak—it must have been manufactured *out of something*. A seed may sprout, the germ may develop, but it is at the expenses of the “albumen” about it and of the water it imbibes. It will not grow

into a plant, and itself produce more seed, unless it be supplied with appropriate nutriment from the soil and atmosphere. So there must be something in the body into which a virus is inserted to nourish it, and permit of its reproduction—a *pabulum*. This pabulum is believed to exist in the blood—the blood which circulates through the frame, whose “home,” where it remains longest, is the capillary vessels reticulated through all the tissues of the body. What is it? It is not anything absolutely essential to the blood, in the sense that it is necessary to that fluid as the source of supply of nutritious matter for the tissues and organs: it is nothing essential to their wellbeing, for, when it is absent, as it is sometimes, from the blood, the body thrives just as well without it. Still it is something natural to the blood, since few people exist in whom it is not present in that fluid. Its actual nature is not understood. All we know of it is, that the growth and multiplication of the virus does away with it, just as the growth and multiplication of the yeast plant does away with the sugar in an infusion of malt.

Now, let me repeat, the virus, in the inoculation of small-pox or vaccinia, is introduced into the capillary vessels and tissue of a certain spot on the skin: part probably remains in the tissue, part is undoubtedly carried into the circulation generally, and, in time, becomes mixed with the whole mass of the blood. In both situations it works. In the tissue of the skin it probably finds its pabulum so elaborated as to be in a condition for immediate appropriation, and it immediately begins to develop itself and multiply. While doing this it modifies either mechanically, or both mechanically and vitally (through the agency of the nervous system?), the mode of nutrition at the spot, and a vesicle or local eruption is produced. But whatever alteration of nutrition takes place, the mechanism of nutrition remains the same: nutrient

material is exuded from the vessels, and some of the products of nutrition are reabsorbed and pass again into the circulation. Hence there is reason to believe (and some of the results of vaccination give force to the belief) that a portion of the virus resulting from its local multiplication is taken again into the circulation, and adds its operation to that originally introduced into the blood upon the mass of the blood in the body.

And what is the *modus operandi* of the virus introduced directly or by "absorption" into the mass of the blood? What I am about to say now applies also to smallpox virus, which has found its way into the circulation by casual contagion. It grows and multiplies at the expense of the material in the blood calculated to afford it nutriment. Let us first follow out this process in the instance of smallpox virus. It is not until the eighth or ninth day that the multiplied virus appears in certain spots on the skin, and the mucous membrane of the mouth. And then it is not in *all* the skin that it appears; it is *in spots* that it appears—in *selected spots*. Why selected spots? why not universally in the tissue of this organ, so abundantly supplied with capillary vessels that the puncture of the finest needle, or even the bite of a flea, causes blood to flow? In the first place, I do not see that any proof has been yet given that, prior to the formation of the general smallpox eruption, the material in the system capable of being transformed by the operation of the particle of virus introduced has really undergone a complete change. When the papules begin to rise, there is found to be a large multiplication of virus which is concentrated in these spots, but it does not follow that, previous to their concentration, it existed as fully formed virus in the blood. Else why is the eruption always deferred until the seventh or eighth day? It looks very much as if there were some intermediate process, some intermediate stages of

imperfect elaboration passed through. Natural history may perhaps supply us with an analogy which, if not perfect, will serve to explain my meaning. We assume, it will be recollected, that the virus is a living being of some kind—"a particle of germinal matter," as Beale would call it. What is known of certain low organized forms of living beings? I will take the entozoa: I might go lower in the scale; but, if this happens with beings so comparatively highly developed, it may well happen with beings far less fully organized. Recent observations of comparative anatomists have shown that there may be many stages of development gone through, from the time that the ovum becomes free to that at which the complete animal is developed, and that these various stages are not transitory; that, so long as the conditions in which the being lives remain unaltered, so long the stage of development remains unaltered; but when the conditions surrounding it are changed, the stage of its development is advanced. Between the ovum and the perfect being—perfect in the sense of being capable of reproduction of its species—it may present itself to the eye of the observer in forms so unlike that nothing but prolonged and laborious investigation has sufficed to show that they are in any way related the one to the other. Take the liver-fluke for an example. First, it appears, after the usual process of yolk-segmentation, as a free ciliated embryo, in which form it attaches itself to the bodies of certain mollusca; but this is not a fluke, nor will it reproduce a fluke. Next it develops in this locality into "a sporocyst" or "redeæ," and within this is developed a progeny of "cercaria" or larvæ; but these are not flukes, nor can they generate flukes. Thirdly, when these escape, as they do after a time, they appear as free swimming tailed animals, and, after swimming about in the water, at last again become parasitic in some mollusc or aquatic insect, become encysted and lose their tails. But in

none of these states are they flukes, or are they capable of reproducing flukes. Lastly their "host," the mollusc or insect that the creature in this stage inhabits, is casually swallowed by some animal, an ox or a sheep; and finding now its last resting-place, the imperfect guest becomes a fully developed fluke, and is reproductive of ova which are capable of passing through similar stages of development. So I hold it may be with smallpox virus. It may not be fully developed except in its last and natural resting-place—the skin. It is here only that it is known as having assumed its perfect character, namely, a virus capable of reproducing more virus. Prior to this time it has probably, like the imperfect fluke in the water or on the body of the mollusc or insect, been undergoing a variety of transformations and multiplications, feeding itself upon what was fitted for its nutriment in the blood, its temporary habitat. What this intermediate condition of the virus, as it exists, thus undergoing multiplication, in the blood, may be, what the intermediate changes undergone by the pabulum ultimately converted into new virus, through the vital agency of the original germ or particle of virus may be, we cannot even speculate. All we know is, that after the lapse of eight or nine days all the pabulum has become changed and converted into something which, when arrested in the capillaries of the skin, appears there as complete virus multiplied indefinitely. And in this condition, in which the multiplied virus exists in the blood by the eighth or ninth day, it is an irritant to the nervous centres through which the blood circulates. In virtue of this quality it produces the nervous phenomena of the initiatory fever which I have described, and determines the localisation of the virus in certain spots in the skin.

I have spoken of this localisation as an arrest of the virus in the capillaries of the spots where it takes place, and of the influence of the nervous centres as determining where these

spots shall be. Now, we know that the nervous centres do thus operate in some other diseases of the skin. Probably the action is a reflex one, and consists in effecting some contraction of the calibre of the capillaries at the selected spots. But no amount of contraction that we conceive of would probably suffice to arrest so very minute a thing as a particle of virus, unless the latter were enlarged in some way; and it is believed to be not unlikely that this occurs by an accretion about it of some material from the blood, perhaps of fibrinous matter, so as to form a little embolus, which would choke up the contracted capillary, and give thus an opportunity for the local development of the arrested particle it contains. At the spot where the virus becomes arrested it attains its complete development (if it had not attained it before), and there it generates until all the transformed material in the blood has been removed or exhausted—"eliminated," as we commonly say;—and thus the nervous phenomena which its presence in the blood occasioned cease, and the fever subsides.

Now, this is the mode in which I believe the virus of smallpox introduced into the skin by puncture operates locally and constitutionally. *How about vaccine virus?* Locally it operates precisely in a similar manner to the virus of smallpox, and in a certain degree constitutionally also. We have seen that, exceptionally, it may produce a constitutional operation, even to the production of a secondary eruption similarly to smallpox. But here, in general, another consideration steps in. *Vaccinia* is not a disease natural to man. It does not thrive in the soil of his body. That soil is foreign to it. The pabulum exists, it is true; it is the same pabulum as suffices for the metamorphosis of smallpox virus; but the climate and other circumstances are unfavourable, and it refuses to feed upon it to the same extent. The virus does not thrive in the blood, although it thrives very well where a quantity is introduced into the tissue at the point of puncture. Some of it operates

for a time, and probably produces an imperfect transformation of the material, but not usually a full transformation—not such a degree of transformation as shall suffice deeply to impress the nervous system, and produce a secondary eruption. Besides, for the transformation to continue in such a soil, for the pabulum to be more deeply transformed, it appears necessary to ensure repeated and larger supplies of new virus. There is a difficulty, and more force is requisite to overcome it. It is the same in the horse. It is commonly said that to the operation of a virus it is quality, not quantity, which is important. True, quite true; but still there may be an exception to the rule. It is most certainly true as regards smallpox virus, the virus of syphilis, of measles, of scarlatina (and even of a virus so energetic as that of glanders), all of which are natural to man when the virus gains access to the blood; but it is not true, apparently, of a weak virus like that of vaccine, when implanted in a foreign soil. Let me refer to the recent experiments of M. Chauveau.* When he inoculated vaccine virus into the skin of the horse, all the effect produced was a local vaccine eruption at the punctured spot; but when he injected vaccine virus *in appreciable quantity* into the lymphatic vessels, or into the blood-vessels, he obtained something more, *a general vaccine eruption*, modified by the constitution of the horse into *horse-pox*. Could we do the same with man—could we inject vaccine virus directly, in appreciable quantity, into his blood or lymphatics—we should probably obtain in him also, as in M. Chounaux Dubisson's patient, a general vaccine eruption modified by the constitution of the subject. Even in the instance of a disease natural to man quantity of virus may be essential to success in producing a secondary eruption—a constitutional result, where the material in the system cal-

* A résumé of his results will be found in 'Archives Générales,' Oct., 1866, p. 495. The Report, in full, is in the 'Bull. de l'Acad. de Méd.,' t. xxxi.

culated to support the vitality of the virus is not in its normal condition. It certainly is not in its normal condition for some time after vaccination has been performed, for the result of a second vaccination, if any at all, is not normal; it is commonly a modified and not a normal pock: and if smallpox be caught, it is a modified and not a normal eruption that follows. In such patients Dufresne, of Geneva, and Chrestien, of Montpellier, found that, although smallpox could not be imparted by a single puncture, they always succeeded when they multiplied the punctures; that is, when they took the only practicable means of ensuring the operation of increased force of transformation—when they multiplied the transforming agency. So we see that there *are* circumstances in which quantity of virus is a matter of importance, and it would appear that this is the case in the instance of vaccine.

The effect of the operation, to the full extent possible in the individual, of the virus of a febrile eruptive disease, is to exhaust the system of pabulum or material capable of transformation; so that the subsequent introduction of a second dose of virus produces and can produce no effect. The system, as we say, is no longer susceptible of its influence. Thus, in a patient who has undergone an attack of smallpox, inoculation of smallpox matter is fruitless. He is *protected*.

Now, the protection afforded by an attack of smallpox extends, not only to smallpox, but also to vaccinia. Moreover, if we produce in any person the vaccine disease, he is protected against vaccine virus. We shall inoculate him with it a second time (within limits of duration of protection) fruitlessly. And not only is he then protected against vaccinia, but he is also protected against smallpox virus; we inoculate smallpox fruitlessly. The protection afforded is mutual, as if the material transformed in the blood in both

24 RELATIONSHIP OF SMALLPOX, COW-POX, ETC.

cases were the same ; indeed, as if the diseases were really the same or near relations—brothers in the same family.

Are they the same diseases or near relations ? This is the next matter to be considered.

The Mutual Relation of Smallpox, Cow-pox, and Horse-pox.

The ox and the horse, as well as other animals with which we are not now concerned, are subject to diseases which have more or less general similarity to smallpox as it appears in the human subject. In the cow we see a mild disease breaking out occasionally, in which there appears an eruption upon the teats, and sometimes upon other parts of the udder, which is the eruption of the cow-pox as we usually meet with it.* It is accompanied mostly with general symptoms of constitutional disturbance, but not always. In India, however, the same disease under the name of Mátá, Goté, &c., assumes a more severe and general character, the eruption not being confined to the teats and udder, and the attacks being sometimes fatal. When this disease, either in its mild or severe form, attacks one of a herd, it spreads by contagion. This is the *varioid disease of the ox*, or cow-pox.

In the horse also, from time to time, a contagious disease occurs, characterised by hot, red, and painful swelling of the hams, due to a pustular eruption, which appears also upon the buttocks, nostrils, lips, vulva, &c., and is accompanied by general constitutional disturbance. Such a disease broke out at Toulouse in 1862. An account of the outbreak and full description of the disease will be found in the "Report on Vaccinations in France for 1863," p. 5. This disease, which some believe has been confounded by writers with

* One of the best descriptions of cow-pox will be found in Steinbrenner's 'Traité Sur la Vaccine,' p. 599. See also Ceely, 'Transactions of Prov. Med. and Surg. Assoc.,' vol. viii, p. 307.

“grease” (*eaux aux jambes*), is the *varioloïd disease of the horse*, or horse-pox.

In order to show the relationship between these diseases and between them and human smallpox, I will state the results which have followed the inoculation of smallpox virus upon the cow and horse, and of horse-pox upon the cow and upon man.

1. *Inoculation of Smallpox Virus upon the Cow and Horse.*

The opinion that cow-pox was the smallpox of the bovine species was already promulgated when Mr. Ceely, of Aylesbury, first undertook his experiments in the inoculation of smallpox virus upon the cow in 1839.

He selected for this purpose three sturks. In his *first* experiment he made seven punctures on the left side of the vulva, and, as there was no appearance of their rising on the ninth day, he vaccinated on the right side of the vulva. On the tenth day, however, some of the variolated punctures were hard and elevated, and *one* had assumed the form and appearance of the vaccine vesicle. From this he took lymph upon points. By the thirteenth day, the fifth of vaccination, the vesicles from the vaccination were sufficiently advanced for lymph to be taken from them. By the sixteenth day both the smallpox vesicle and the vaccine vesicles had begun to wither. By the twenty-fifth day the crusts had all fallen, and scars were left which agreed in appearance, except that those from the vaccine virus were less deep and smaller, and had more induration about them than that from the smallpox virus. No result was obtained when he reinoculated the two kinds of virus several days afterwards. In his *second* experiment he merely inoculated smallpox virus, and as, by the tenth and eleventh days, nothing but lymphless tubercles had resulted, he reinoculated a few days later with seventh or

eight day virus, deluging the punctures with it, and leaving points in the punctures to induce some irritation. He made eight punctures, which had evidently taken on the fifth day, and all of which on the sixth day presented the appearance of the vaccine vesicle. From one of these he charged thirty-nine points. By the eleventh day the vesicles were beginning to decline, and on the twelfth day he vaccinated the animal, but without result. At a later period he revaccinated and reinoculated smallpox also without result. The *third* experiment was a failure. He specially states that there was no fever accompanying the eruption.

From the lymph thus obtained with smallpox virus, Mr. Ceely vaccinated several children, and perpetuated the supply in successive human generations. Several surgeons used it, and it was also in use for many months at the Smallpox Hospital and the Cow-pox Institution of Dublin. There can be no question, then, about the fact that cow-pox was produced by the inoculation of the smallpox virus. Mr. Ceely failed in infecting cows after the manner of Dr. Sonderland by covering them with the clothing and blankets, &c., of smallpox patients, but these cows were equally insusceptible of vaccination. In 1840 Mr. John Badcock,* then of Brighton, in ignorance, as he informs me, of Mr. Ceely's success, commenced a similar series of experiments, which he conducted on a large scale at great pecuniary expense. He has succeeded in variolating cows forty or fifty times, but the number of attempts he has made may be judged of by the fact that these successes only form about seven per cent. of the trials. It is by no means an easy thing, then, to effect. Mr. Marson, he tells me, attempted to variolate forty cows in succession, but met with nothing but failure. Still, from lymph thus obtained Mr.

* I am indebted to Mr. Badcock's kindness for replying to my application to him for information, and also for a pamphlet containing the opinions of a large number of medical men who have used his lymph.

Badcock has himself vaccinated upwards of 20,000 persons, and he has distributed it to more than 500 medical men at home and abroad, who were all well satisfied that, in using it, they were using true vaccine lymph.

Within the last year, again, a successful inoculation of this sort has been effected in France by Dr. Vy, of Elbeuf,* who, on using the lymph, obtained the same result upon the human subject as in using vaccine virus. According to M. Depaul, the same experimenter has succeeded in inoculating a lamb with smallpox virus, the pustule produced furnishing a liquid which he inoculated on an infant, producing an eruption with all the characters of the vaccine vesicle.

These are the more recent instances of success; but although from the difficulty of the operation many experimenters, as Coleman, Ring, Dalton, Sacco, Fiard, and Bousquet, had all failed in their attempts, some had succeeded before Ceely. Thus, in 1807, Gassner succeeded, and vaccinated infants from the pocks produced. In 1836, Theilé, of Kasan, succeeded after several previous failures, and again used the pocks for vaccination, and at the time he wrote about it had transmitted the virus through seventy-five human generations. He, as other observers have found to be necessary, adopted a number of minute precautions, especially as to the age of the animal to be inoculated, the removal of hair, the selection of active smallpox virus, &c. Reiter, of Munich, also, after former failures, succeeded on adopting Theilé's method.

Experiments so positive as those of Ceely and Badcock cannot be overturned by the negative results obtained by the Commission recently appointed by the French Academy. The Commission consisted of MM. Chauveau, Viennois, and Meynet, who gave in their report in 1865.† They say, "1st.

* 'Bulletin de l'Acad. de Méd.,' t. xxxi, p. 430.

† 'Archives Générales,' sixth ser., t. vi, p. 104.

That human smallpox may be inoculated upon the ox and horse with the same certainty of effect as the vaccine virus. 2nd. That the effects produced, however, by the inoculation of these two kinds of virus absolutely differ. In the case of the ox, smallpox virus only produces an eruption of papules so small that unless a person were forewarned of their existence they would pass unperceived. Vaccine virus, on the other hand, produces a true vaccine eruption, with large and well-marked pustules. In the case of the horse, smallpox virus produces, as in the ox, nothing more than a papular eruption without secretion or crusts, and, although this eruption is much more marked than in the ox, it could never be confounded with horse-pox, which is remarkable for the abundance of its secretion and the thickness of its crusts." Probably the results would have been more marked had they continued their experiments longer, for Ceely notices in his cows that some of the punctures only occasioned what he terms "tubercular" elevations—I presume the same result as the French Commission obtained. They go on to say, however, "3rd. Vaccine virus inoculated into animals of the horse and ox species preserves them from the operation of the virus of smallpox. 4th. Smallpox virus inoculated into these same animals imparts an immunity from the operation of vaccine. 5th. Although the virus of smallpox be assiduously transmitted through a series of oxen or a series of horses, nothing like vaccinia is developed on either." I do not know how this could be expected. The eruption they obtained was clearly a modified and imperfect eruption.

2. Inoculation of Horse-pox upon the Cow and upon Man.

Dr. Jenner* held the opinion that cow-pox originated, in the Gloucestershire dairies where it broke out, from infection

* Jenner, 'An Inquiry into the Causes and Effects of the Variolæ Vaccinæ,' &c., p. 23.

carried by the men engaged in milking, and who had been attending upon horses affected with the disease termed "grease." Several of the cases he narrates in his first work have a bearing upon this subject. His tenth case is of this character:—"Simon Nichols lived as a servant with Mr. Bromedge, who resides on his own farm in this parish, in the year 1782. He was employed in applying dressings to the sore heels of one of his master's horses, and at the same time assisted in milking the cows. The cows became affected in consequence, but the disease did not show itself on their nipples till several weeks after he had begun to dress the horse. He quitted Mr. Bromedge's service and went to another farm without any sores upon him: but here his hands soon began to be affected in the common way, and he was much indisposed with the usual symptoms. Concealing the nature of the malady from Mr. Cole, his new master, and being also employed in milking, the cow-pox was soon communicated to the cows. Some years afterwards Nichols was employed in a farm where the smallpox broke out, when I inoculated him with several other patients, with whom he continued during the whole time of their confinement. His arm inflamed, but neither the inflammation nor his associating with the inoculated family produced the least effect on his constitution." Again:* He relates the case of three men infected with sores on their hands from dressing a mare with sore heels. One of them was daily employed as a milker at the farm, and cow-pox began to show itself among the cows about ten days after he first assisted in washing the mare's heels. From another of the men thus affected Dr. Jenner inoculated a child (Case 18) with the production of an effect similar to that produced by cow-pox matter. Another child was inoculated with matter taken

* Jenner, 'An Inquiry into the Causes and Effects of the Variolæ Vaccinæ,' &c., p. 35.

from the cow, and the ordinary vaccine disease was communicated. Dr. Jenner says that "with regard to the opinion adduced, 'that the source of the infection is a peculiar morbid matter arising in the horse,' although I have not been able to prove it from actual experiments conducted immediately under my own eye, yet the evidence I have adduced appears sufficient to establish it." *

This doctrine, foreshadowed by Jenner, was subsequently propounded by Dr. Loy.† In 1801 he found upon the hands of two persons at Pickering, in Yorkshire, who had been engaged with horses affected with "grease," an eruption resembling vaccinia. With the matter derived from these persons he inoculated two persons, and obtained vaccine pustules. He also inoculated some of the same matter upon a cow who had a fine pustule with all its accessories, and from the cow he transmitted the virus to a child who had a fine vaccine pock. On testing this child by means of smallpox inoculation on the sixteenth day he obtained no result. Again, using a horse with a recent eruption, he inoculated five cows, and succeeded in all, vaccinating infants from the pocks, and testing their protection afterwards with smallpox virus. When he used matter from an affection which had lasted upon the horse for a longer period, he failed in obtaining the result.¹

Possibly the failure of other observers in the inoculation of the matter of grease may have been due also to the late period of the eruption at which it was taken. The subject has been much discussed lately in the French Academy, and it has been thought by some that "grease" and the horse-pox which is allied to vaccine disease are not identical affections. I cannot pretend to decide this question; I must

* Jenner, 'An Inquiry into the Causes and Effects of the Variolæ Vaccinæ,' &c., p. 45.

† 'An Account of some Experiments on the Origin of Cow-pox,' by John G. Loy, M.D.

content myself with stating what has been observed. Several diseases may be confounded together under one common appellation, one of which may be the true varioloid disease of the horse.

Sacco thus inoculated the matter of "grease" into eight infants and twenty-seven cows without success. But, for all that, he had seen it form sores upon the hands of persons attending upon horses thus affected. Taking lymph from these persons he inoculated nine children and a cow: in two of the nine children he got a normal vaccine vesicle.

Ritter, of Kiel, also, has seen the disease arise upon the hands of persons engaged in treating horses with "grease," and has inoculated infants from them, producing normal vaccine.

Dr. Stokes, of Dublin, also once found vaccine vesicles, or what resembled them very much, upon the lip of a man who, having wounded his lip, wiped the wound with the sponge that he was using for the horse.

I am not going to discuss whether cow-pox always arises from infection from the horse, or whether this disease is "grease" or some other affection. There is every reason to believe that, whatever the relation may be between the affection on the horse and cow-pox, the latter sometimes has arisen in dairies where no contagion from the horse has been traceable. Besides, recent observations made in France have established, I think very conclusively, that there is a disease in the horse which may properly be called horse-pox, which is capable of being inoculated both upon the cow and upon man, and produces in both an eruption which is undistinguishable from cow-pox or vaccinia, which protects against the virus of the vaccine disease, and also against the virus of small-pox.

In 1860, a contagious disease broke out among horses at Rieumes, not far from Toulouse, having the characters I

have briefly described above as those of "horse-pox." From a pustule on one of these animals M. Lafosse inoculated a young cow with the result of producing pustules, which both he and M. Prince, the president of the commission appointed to investigate the outbreak, recognised as an eruption of cow-pox, only differing from that produced by the inoculation of cow-pox from one cow to another, in the fact that the latter occasioned finer pocks. M. Cayrel now inoculated an infant from the horse, and obtained six magnificent vaccine pustules. In order to make sure of the comparison with vaccine being fair, he inoculated one arm with the horse-pox and the other with ordinary vaccine virus. Further, upon children inoculated from the horse-pox, he reinoculated with the ordinary vaccine without result, thus showing the protection they had received. More than 200 infants were inoculated with the new virus. It is worthy of remark that at the time the horse-pox broke out at Rieumes, small-pox was very prevalent among the human population. In 1863, again, M. Bouley* of Alfort presented to the Academy an infant vaccinated with cow-pox proceeding from a cow on which he had inoculated the serous liquid of vesicles, apparently aphthous, developed spontaneously in the buccal cavity of a horse.

What conclusion, then, are we to derive from these positive observations. That smallpox, cow-pox, and horse-pox are identical diseases? Certainly not. If they were, they would present identical symptoms. It is only through the symptoms of diseases (allowing for individual modifications) that we recognise and classify them. That they are different diseases arising in different animals from the operation of the same virus? Again, no; for if this were strictly true the vaccine virus would produce smallpox in man, and so too

* 'Archives Générales,' sixth ser., t. ii, p. 240.

would the horse-pox virus. What then? I think that the proper mode of stating the matter is this:—1st. That smallpox, cow-pox, and horse-pox are different diseases, each being natural to man, the bovine species, and the equine species respectively. 2nd. That the viruses of these several diseases differ, being the specific products of the several animals in which these diseases naturally occur. 3rd. That when the virus, “germinal matter,” “contagium” of human smallpox, is inserted into the skin of a cow, it finds there material capable, indeed, of maintaining its life and permitting of its generation, but only in a modified manner—so to speak, in a degraded manner. The system or skin of the cow is unfitted for the development of smallpox; it can only, by virtue of its natural constitution, develop cow-pox; and as smallpox virus operates upon the same constituents as cow-pox virus, cow-pox is the disease produced. 4th. That it is the same when the virus of horse-pox is inoculated into the skin of the cow. 5th. That, *mutatis mutandis*, the same explanation must be given of the effect produced when cow-pox matter is inoculated into the skin or injected into the vessels of the horse. Horse-pox is the disease which this animal alone can develop, and horse-pox is developed. 6th. That the constitution of the higher animal, man, is such that he is capable of developing both his own special “varioid disease,” smallpox, and the varioid disease of the cow, but is incapable of developing the varioid disease of the horse. Still the virus of each, when inserted into his system, finds there the material for maintaining its vitality and permitting of its generation. That the virus of smallpox, “degraded” in the generation it has undergone in the cow, does not recover itself again when transferred to the human system.* It

* Dr. Beale writes thus: “It seems to me that ‘germinal matter’ may lose formative power and become degraded, and cannot acquire or regain it when lost. There is, as it were, no return to a high position of living matter which

operates upon the material in the blood that smallpox virus operates upon, and produces thereupon a similar but probably less complete change to that which smallpox virus produces. It is the same with horse-pox virus. In either case it is cow-pox which is produced; but protection is equally afforded, whether the virus be taken from the cow or from the horse, both against cow-pox and against smallpox.

has once suffered degradation, nor can degraded germinal matter produce descendants with exalted power.”—‘Appendix to the Third Report of the Cattle Plague Commissioners,’ p. 148.

PART I.

“THE ACTUAL VALUE OF VACCINATION AS A PREVENTIVE OF
SMALLPOX, EMBRACING, IN THIS PART, THE QUESTION
WHETHER VACCINATION AS A PREVENTIVE RETAINS ITS
POWER.”

ACTUAL VALUE OF VACCINATION.

“It now becomes too manifest to admit of controversy that the annihilation of smallpox, the most dreadful scourge of the human species, must be the final result of this practice.”

It was thus that the discoverer of the protective power of the vaccine disease expressed himself in 1802, four years only after the time when he first made the announcement of it to the world. Dr. Jenner's sanguine hope has not been fulfilled. Experience has not verified his prediction. Smallpox has not been eradicated. Let me add that scientific observation and reasoning give no countenance to the belief that it ever will be eradicated, even from civilised communities. But although we must assume for vaccination a more humble position—although we must detract somewhat from its pretensions—it nevertheless stands upon a pedestal far above that upon which any other prophylactic measure ever proposed against the invasion of a serious malady can fairly be placed. The following propositions are capable of proof:

1. That the practice of vaccination is worthy of confidence as a protection against attacks of smallpox.
2. That the protection it affords is neither unconditional nor constantly unlimited, but that many of the conditions upon which it depends are under the control of mankind.

THE PRACTICE OF VACCINATION IS WORTHY OF CONFIDENCE AS
A PROTECTION AGAINST ATTACKS OF SMALLPOX.

The very fact that confidence is generally imposed in vaccination, among the best educated and the best informed classes of the community, is in itself a *primâ facie* proof that it is not misplaced. Very few indeed of the upper and middle classes of society in this country decline to avail themselves of this protection for their children. On the other hand, in seasons when smallpox has prevailed epidemically, the demand for *revaccination* has now and then become so extensive, that the primary vaccinations performed by private practitioners have not sufficed to furnish the necessary supply of vaccine lymph. I have already said that I do not regard this public confidence as always very enlightened, but it may be presumed to have a solid basis in the case of the members of the medical profession. They, at all events, cannot be supposed to put faith in that which has no claim whatever to be relied upon by reasonable, thinking, and scientific men. In the present day, when dogmas of all kinds, however sanctioned by tradition, are subjected to renewed discussion, when the authority of great men is only accepted as a ground on which their opinions are held to be worthy of being tested, when fact and experience reign supreme, very few indeed of this class are to be found who maintain the general inefficacy of vaccination. On two recent occasions the opinions of the medical profession upon this subject have been officially gathered in; namely, once by the Epidemiological Society, in 1852, and once by Mr. Simon, the medical officer of the then General Board of Health, in 1856. The questions put by Mr. Simon were addressed not only to British practitioners, but to medical men and to Governments abroad. The unanimity of the replies in regard to the general value of vaccination suffi-

ciently established the favorable opinion which the experience of medical men at large had enabled them to form. The 539 replies printed by Mr. Simon,* bearing, among others, the names of Brodie, Williams, Erichsen, Farr, Alderson, Addison, Barlow, &c., at home; and those of Rayer, Bamberger, Chomel, Moreau, Rostan, &c., abroad, are well worthy of the perusal of any one with whom such an argument as this carries any force.

But there are many persons with whom such an argument would carry little force, or who would be disposed to decline its acceptance altogether. Such persons are apt to fall back upon the history of the past, and, ignoring altogether the weighty difference between the method of scientific inquiry pursued universally now, and that which was commonly adopted formerly, are apt to quote the fluctuations of medical opinion, from epoch to epoch, as among the most remarkable examples of scientific uncertainty. It consequently becomes incumbent upon me to point out some of the principal grounds upon which medical opinion is at present founded, and to offer, for the consideration of my readers, a few of the most remarkable illustrations of the general value of the practice of vaccination from among the many that are ready to my hand. In the selection of these illustrations I shall, in all cases, prefer those which, having been published at different times since the discovery of vaccination, are capable of being easily verified by reference to the works from which they are extracted.

The first and the most conclusive and unexceptionable test that could be applied, of the immediate protection against smallpox, afforded by an individual having undergone the vaccine disease, was that applied by Dr. Jenner and his early followers. Having vaccinated a person successfully, and the

* 'Papers,' &c., pp. 32—117.

disease having run its course, they *introduced smallpox virus* taken from a smallpox pustule, by the ordinary method of *inoculation*, into the absorbing structure of the skin. Under ordinary circumstances, as I have shown, the result would have been in all but a very small proportion of individuals thus treated, first the production of a local pustule, and then a general outbreak of smallpox over the body. But in vaccinated persons thus tested nothing of the kind took place. Persons who had recently taken cow-pox by contagion from the cow, and those to whom it had been given by vaccination, were equally protected from the operation of the virus of smallpox when subsequently inoculated. The protection, moreover, was found to be the same, whether the vaccine virus were taken from the cow directly or from one of the human species upon whom it had been transplanted, and in whom the disease had been regenerated. That persons who had accidentally received contagion from the cow were exempt from smallpox was a tradition in Gloucestershire at the time when the report met the ear of Dr. Jenner. His first experiment in vaccination was made upon a lad of the name of Phipps,* upon whom he inoculated the virus taken from the hand of a young woman accidentally infected from a cow. Some months afterwards Dr. Jenner inoculated the same lad with smallpox matter, and again after the expiration of five years, without producing smallpox upon him on either occasion. The first work which he published upon the subject, in 1798, contains a record of twenty-three cases, as well as several additional ones. It contains an account of unsuccessful attempts to inoculate smallpox upon dairy servants and others, who had, at various periods of their lives, received cow-pox by contagion: and amongst the cases adduced there are several where persons similarly infected previously had

* 'On the Origin of the Vaccine Inoculation,' 1801. Reprinted pamphlet.

escaped an attack of smallpox under exposure such as might have been expected, in ordinary circumstances, to have imparted the disease. He obtained similar protection against inoculation of smallpox and casual infection, by inoculating cow-pox matter directly, or after previous transmission through the human subject; and the observations of various correspondents, published in his subsequent treatises, were all of a character to confirm those which Dr. Jenner himself made. It is difficult to comprehend how Dr. Jenner could have arrived at any other conclusion than "that the cow-pox protects the human system from the infection of the smallpox." Of all tests that could be applied none could be more satisfactory and more free from exception than the direct introduction of smallpox virus, inasmuch as no question could arise as to whether or not the vaccinated individual had been honestly subjected to the chance of receiving that malady.

But the observations of Dr. Jenner and his earliest correspondents were not left without abundant confirmation, and that too by experiments instituted upon a large scale. In the year 1799 Dr. Woodville was the physician to the Smallpox and Inoculation Hospital, in London, and had the good fortune to meet with cow-pox in a dairy in Gray's Inn, where a milker of the name of Sarah Rice had become infected.* On January 21st in that year he inoculated seven persons, by a single puncture, from the teat of the cow, and subsequently endeavoured, fruitlessly, to impart smallpox to them, both by inoculating the virus and by exposing them freely to the influence of contagion. Within the period of two years Dr. Woodville transmitted the disease, by successive generations,† from one person to another, to 7500

* 'Observations on the Cow-pox,' 1800, p. 10.

† I purpose using the term "generation" as expressing the renewal of virus in passing through a single individual. Thus a person vaccinated from the cow would give a virus of the first generation, the person vaccinated from the individual one of the second generation, and so on.

individuals, about a half of whom he subsequently inoculated with variolous matter without success. In addition to this he exposed many of them very freely to the operation of smallpox contagion. Similar experiments, always with the same absence of result, were made in France by MM. Chaussier, Pinel, Husson, Salmade, Jadelot, and others, at the Salpêtrière, the Maternité, and other institutions, with which they were connected, as well as by equally distinguished medical men in other continental countries. In 1804, when public confidence had been shaken to some degree by the rumour of instances in which smallpox had occurred notwithstanding a previous vaccination, Dr. Pearson repeated the experiment of smallpox inoculation upon sixty vaccinated individuals, but again without imparting the disease; and, at various periods of the history of vaccination, others have, over and over again, subjected the protective power of the vaccine disease to the like ordeal. Thus, in Mr. Cross's book,* we find several of his correspondents stating that they had inserted smallpox matter into the arms of persons vaccinated fifteen to twenty years previously, without producing in them anything more than a local disturbance, or a very trifling general eruption, to which the term "smallpox" was scarcely applicable; and, in many instances, without producing any effect at all. Even in India, we learn from Dr. Duncan Stewart,† that the protective power of vaccination has been similarly proved, although on a much smaller scale. In 1841, we read that Asst.-Surg. Russel inoculated with smallpox matter six natives, aged seven, nine, twelve, seventeen, and twenty-one years, who had all been vaccinated when very young, but that the smallpox inoculation proved a failure. I mention this

* 'History of the Variolous Epidemic in Norwich in 1819,' &c.

† 'Report on Smallpox in Calcutta, 1833-44, and Vaccinations in Bengal, 1827-41,' p. 163.

instance particularly, because the natives of India exhibit, as the result of vaccination, vesicles much smaller than those which appear after the operation in the European races.

It is quite unnecessary to pursue this matter further. I shall therefore go on to show the protective influence exerted by the vaccine disease in protecting individuals, families, and neighbourhoods, from *the operation of smallpox contagion*. Many of the illustrations that I shall select will be of the nature of the *experimentum crucis*. I shall avoid using any where a reasonable doubt can arise in any mind as to the reality of the exposure to the action of the smallpox infection, and shall draw them from sources to which no candid mind can object, and from the narrations of medical men whose testimony cannot be invalidated. Neither shall I confine myself to recent observations, on the one hand, or to old observations on the other, inasmuch as it is my object to show that, what held good during the early days of the practice of vaccination, has held good ever since, and holds good now.

I have already mentioned the fact, that some of the earliest vaccinators took *great pains to give smallpox by contagion to persons who had recently undergone the operation, but invariably without success*. The replies made to Mr. Simon's appeal to the profession furnish abundant testimony in the same direction. Thus, Mr. M'Munn, of Wolverhampton, relates a case in which he vaccinated successfully an infant at the breast. The mother, who had never been vaccinated, was seized with confluent smallpox three weeks afterwards, and although for a period of four weeks the infant was exposed to the contagion, remaining at the breast all the time the mother was ill, and actually sucking the matter off the pustules around the nipple, it continued to grow and

throve well, not suffering the least from the mother's illness.* Mr. Earle, of Cromer, narrated a similar case to Mr. Cross,† in which a woman, who had been inoculated for smallpox twenty years previously, suffered in 1819 from a second attack of the disease. This woman had an infant at her breast who was vaccinated a few weeks before, but who remained unaffected by the mother's disease. He further states that, during an epidemic period, not a single patient of his who had been previously vaccinated suffered from the prevailing contagion, "although great pains were taken to make them do so." Mr. Cross was informed also by many of his correspondents, that they had repeatedly vaccinated children in the same house with smallpox; and, although they often lay in the same bed with patients suffering from the disease, they failed to take it. Bousquet‡ narrates the instance of an infant who, having been vaccinated a month previously, had for a bedfellow and foster-brother a child which died from smallpox; and what renders this fact of greater importance is, that the nurse had but one breast that she could use for suckling both children. He mentions also another case where a nurse, although covered with smallpox, suckled two vaccinated infants. In none of these instances was the smallpox communicated. The same writer states that, on the first introduction of vaccination into France, not only was tentative inoculation with smallpox virus practised, but infants who had been thus unsuccessfully inoculated were designedly brought into close relation with other children suffering from smallpox, shut up with them in the same room, put into the same bed, covered with their clothes, made to eat out of the same plate and drink out of the same glass, and all without in any instance imparting to them

* 'Papers relative to the History, &c., of Vaccination,' p. 86.

† Cross, *op. cit.*, p. 273.

‡ 'Traité de Vaccine,' p. 114.

smallpox. The following series of events related by Mr. Johnson, of Sunderland,* is instructive. It occurred in a family consisting of a man and his wife, four children, and the man's brother. "The wife, on whose arm I could not find any cicatrix, although she thought she had been vaccinated, took confluent smallpox, aborted and died. Her husband, not vaccinated, received the disease in the same form and recovered after a severe illness. The brother, who was vaccinated, became infected,† and had twenty or thirty imperfect pustules, conical, and with no central absorption (depression?). The four children all escaped, all having been vaccinated. Again, a man visiting the above, and not being vaccinated, took smallpox. His wife complaining of suspicious symptoms, I revaccinated her and nothing further occurred, two children in this man's family also escaping. In another instance a sailor, after being three weeks at sea, was attacked with confluent variola: he had not been vaccinated, his wife and two children had, and the children and wife were not infected. These three groups occurred to me simultaneously during last month; and, strange to say, they are the only cases of smallpox I have attended, in a tolerably extensive practice, for nearly five years."

So much for narrations of domestic events. Those I have adduced could be "capped" by nearly every medical man in active practice, especially among the poor. I will merely mention in addition, as an important evidence in favour of the protective power of vaccination, that some few years ago the Epidemiological Society issued an inquiry addressed to a number of medical men, who were known to be in the way of frequently having to treat cases of smallpox, as to the amount of protection from the contagion of the disease

* 'Papers,' &c., see p. 76.

† I shall discuss at a further period the occurrence of *post-vaccinal* smallpox.

which they had *personally* experienced. Out of 429 who replied to the inquiry, there were 347 who had undergone vaccination, the remaining 82 having been inoculated for smallpox in their infancy.* Although so closely and frequently exposed to contagion as these gentlemen must have been, out of the 347 who had been vaccinated, only 44 or 12·6 per cent. had taken the disease, and, with the exception of four, all of them had had it in an extremely mild form. Again, at a meeting of the Medical Society of London, when about sixty practitioners were present whose average age was somewhere between thirty-five and forty-five years, those who had taken smallpox after vaccination were requested to hold up their hands, and of the whole number only four or five were raised.† All of them, however, had repeatedly exposed themselves to infection; and in the case of those who had contracted smallpox, the proofs of true vaccination were not adduced.

We may now extend our view a little, and deal with *groups of persons* rather than with individuals. In that which follows I shall have frequent occasion to allude to epidemic outbreaks, to times when the virus of smallpox has been more or less extensively diffused, and to seasons which have been especially favorable to its reception and to its operation upon the human system. At such seasons, all persons are more liable to attack than in times when the disease is not epidemic; first, because the foci of infection and the opportunities of taking the disease from the sick or convalescent, from contaminated clothing, from the casual use of public carriages which have conveyed smallpox patients, and from fifty other sources, are multiplied; and next, because at such times other conditions are or have been operating (some, perhaps, atmospheric) which promote in some way the diffusion

* Seaton, 'On the Protective and Modifying Power of Vaccination,' p. 8.

† 'Papers,' &c., p. 71.

of the malady, or predispose the system to suffer from it. At such times especially it is that persons imperfectly protected by vaccination are liable to be attacked, although, as we shall see after a time, mostly with a mild and modified form of the disease. Yet on the whole, and taking the vaccinated and unvaccinated in mass, the former are remarkable for escaping that which the latter are little likely to be able to avoid.

Perhaps I cannot do better than illustrate this truth by reference, first, by an inquiry carefully instituted by Mr. Cross* during an *epidemic* of smallpox which broke out with severity in Norwich and its neighbourhood in 1819. At that time he made an entry in a register of the effects produced by the contagion in 112 families into which smallpox was introduced. These 112 families were made up of 603 individuals. Of the 603—

297 had had smallpox before, and they all escaped attack.

91 had been *vaccinated*;—of these only 2 caught smallpox, and they had the disease in a modified form. Both recovered.

215 were *unprotected* altogether;—of these 200 were attacked with smallpox, 46 of them died of the disease, and of those who recovered 3 had one eye completely destroyed, and in 2 more the eye was left damaged. The other 15 escaped; 10 of these had been exposed with impunity before to the contagion of smallpox.

Something more has to be said about the ninety-one who were vaccinated. Thirty-four of them were vaccinated during the epidemic and after smallpox had broken out in their families; but the remaining fifty-seven had been vaccinated at various periods previously, varying from three to twenty years.

3 had been vaccinated from 3 to 5 years previously.

37	“	“	5 to 10	“
11	“	“	10 to 15	“
6	“	“	15 to 20	“

* Cross, op. cit., pp. 7 and 34.

It is to be observed that there were ten of the 215, that is, about 5 per cent., who, although on two occasions at least exposed to smallpox contagion, yet failed to contract the malady.

It may be as well to take this opportunity to make a few observations upon the immunity which some persons appear to enjoy in this respect. It is necessary to say something upon it, because it has been argued that, prior to the use of vaccination, there were always to be found persons thus privileged. I am not going to deny that such occasional immunity is to be met with. Cross estimates the proportion of persons thus privileged at one in fifty, Haygarth and Sauvages at one in twenty, and so on; all it is necessary to comprehend is that the proportion is not large. Van Swieten knew a physician who, although necessarily having had frequent charge of smallpox patients, had never contracted smallpox, although he had arrived at the age of seventy years. Diemerbroeck believed that his was a privileged family in this respect; his father, grandfather, and grandmother did not contract it, and he believed that he himself was similarly exempt. But even a very prolonged exposure does not prove that there is no power of receiving the disease, for Cross narrates a story, told him by one of the Suttons, of a man who lived as a nurse in one of their establishments for inoculated patients for twelve years, and although exempt from attack for all that time, at last took smallpox and died; and cases are on record of persons who have for the first time been attacked with smallpox at an advanced age. Mr. Marson* thus relates the case of a woman aged eighty-three who was admitted into the Smallpox Hospital in 1844 with severe confluent natural smallpox, of which she died, who had nursed her own children and her grandchildren with the

* 'Med.-Chir. Transactions,' vol. xxxvi, p. 388.

disease, and had otherwise been exposed often to variolous infection, but never took it before. The actual proportion of persons who really have no capability of developing smallpox is, perhaps, best represented by the proportion who are incapable of being infected by smallpox inoculation, and Woodville estimated this proportion as one in sixty. One cannot help thinking that the proportion of persons capable of resisting the action of casual contagion must be larger than this; I do not mean of prolonged and repeated exposure. For receptivity under casual contagion is a more complex affair than when smallpox is directly inoculated. In the latter case the virus is actually placed upon the open mouths of divided vessels, and can scarcely fail to enter the blood. "Non-receptivity" under such circumstances means an incapability on the part of the general system to undergo the modifications which the virus brings about in the majority of persons. But non-receptivity on exposure to the contagion may mean something more. The virus, before it can act upon the system, must get into it, and the difficulty may be in this stage of the process.* Dr. Beale, as I gather from his various writings, seems to think (and I am of opinion that he has made out a considerable probability that this is the case) that not only the virus of cattle plague, but the contagious element of other diseases, consists essentially in a living material—"germinal matter," capable of growth, of change of form, and of generation, whose manifestations of life may under certain circumstances be suspended, and which under other conditions may cease to live altogether. Now, we can very well understand that, if this be so, the virus on arriving at a mucous or other surface, such as that of the mouth, lungs, or nostrils, may meet with conditions there

* Dr. Gregory relates the case of an old lady, aged 83 years, at Salisbury, who had brought up a large family, and yet was successfully inoculated for smallpox in 1804.

unfavorable to its life, and so of its reception into the blood, to which it would make its way, under ordinary circumstances, through the walls of the minute vessels of the part. I do not say that it is so, only that this explanation may be suggested upon the hypothesis of Dr. Beale. Certain it is, however, that in respect of all contagious diseases, smallpox among the number, a receptivity to contagion may exist at one time and not exist at another. It is so with scarlet-fever, and with measles, as the experience of every medical man who practises in private families will testify. Either of these diseases may prevail in a family of children and spare one of the number, although living, playing, and eating with the sick ones: but after a year or so, or even longer, this one may suffer after exposure to some casual infection. Very early age appears to confer non-receptivity for some diseases. Thus it is by no means a common thing to see an infant at the breast suffering from scarlet fever; and although smallpox may be seen sometimes upon an infant born of a mother labouring under the disease, yet it also happens that the foetus is unaffected, and new-born infants are known to resist smallpox contagion remarkably during the first two or even three months of their life.* Several examples of this special immunity are cited by Steinbrenner.† But such a merciful immunity does not last, for everybody knows that young children are the most frequent of the victims of smallpox. I shall have to recur to this subject, and only allude to it in reference to one of the objections sometimes taken to the proofs of the protective power of vaccination.

To return, then, to my subject. Cross, at the close of the Norwich epidemic, which he describes, visited 500 poor families, and states that amongst them he met with seventy-seven where vaccinated persons lived in the same room with per-

* This may be due to their being less exposed to the chances of contagion.

† Op. cit., p. 471.

sons suffering from smallpox. Everybody acquainted with the mode of life of the very poor will understand what this means, and will comprehend the sort of exposure to which they were subjected. Yet none had any serious disease, and not above one in thirty had any kind of eruptive disease at all.*

Numerous instances of *exemption from prevailing smallpox in consequence of the careful vaccination of the inhabitants of a district* are related by Dr. Seaton.† Some of the more striking of these I will quote.

In the year 1849 smallpox invaded the village of Road, near Chippenham, a wretched, ill-vaccinated place, and it attacked there forty-eight out of 800 inhabitants. Four villages, all within two miles of it, with populations of 1200, 230, 190, and 170 respectively, and all in constant communication with it, but where the poor are in better circumstances, and all vaccinated, entirely escaped. Mr. Livett of Wells instances the case of a village from which, by good vaccination, smallpox had been entirely excluded ever since 1837, although on several occasions it had prevailed in the villages round about and also at Wells, four miles distant. That it was not the mere hygienic condition of the village, or the want of communication, which, exclusive of vaccination, shut out smallpox, is shown by the fact that other infectious diseases against which no similar protection exists, were not prevented from entering and spreading among the population. A similar instance in which good vaccination protected a village from the spread of smallpox, actually brought into it, is related by Mr. Audland. He describes the village of Brockwin as a low dirty village, the inhabitants of which are poor people, but generally vaccinated. It appears that a case of smallpox was brought into this place from Bristol. The subject was a

* Cross, op. cit., p. 38.

† 'Parliamentary Papers,' April 26th, 1853, p. 8.

young man who had not been vaccinated, and he died. Two women, who attended to him and had had smallpox before, both had, on this occasion, a few pustules; but no other case occurred in the village: the disease did not spread. He relates a similar occurrence on another occasion, and yet, when typhoid fever was prevalent in Bristol in 1850, there was scarcely a family in this place that escaped that disease. Another instance of similar exemption is mentioned by Mr. Steel of Blaenavon, near Abergavenny, who exerted himself actively among a mining population of about 5000 persons in keeping well up the vaccination of his district; and he states that, from 1840 to 1851, there only occurred there twenty-six cases of smallpox, although it had raged virulently all around.

A remarkable contrast in the operation of the contagion of smallpox among a well-vaccinated and badly-vaccinated population is found again in what took place in the epidemic of smallpox in Iceland in 1839.* In the town of Reikavick, the epidemic found a population of 1200 or 1300 persons, most of whom were vaccinated, and here only fifteen deaths from smallpox took place; while, on the other hand, in a fishing cove where there was a population of no more than 600, and where vaccination had been greatly neglected, forty persons died of the disease.

Again, *schools and colleges and the military, when well vaccinated*, have been preserved from the disease that was raging among the people all around them. Thus, in the Marseilles epidemic of 1828 (one of those, by the way, which Hamernjk refers to as illustrating the uselessness of vaccination), terrible as the epidemic was throughout the department, it did not enter the college; and the same author† who relates this fact says also that a similar thing happened in the epidemic of Blois in 1826, when all the people of the college escaped

* 'Papers relating to the History, &c., of Vaccination,' p. 174.

† Bousquet, 'Traité de Vaccine,' p. 114.

attack, except one—the only one who had not undergone vaccination. Dr. Balfour* again gives us the result of an epidemic of smallpox which occurred in Malta in 1830—1, and again in 1838. The military are a protected class, vaccination being carefully attended to and recruits being vaccinated on enlistment. During the eighteen months that the first of these epidemics lasted, the ratio of deaths per 1000 of the military from smallpox was 0·6, only two deaths having taken place in a body of men 2299 strong. On the other hand, among the civil population of the island, amounting to 101,962 persons, 1169 deaths from smallpox occurred, or 7·6 deaths in the 1000. In the twelve months of the second epidemic, not a single death from smallpox occurred amongst the military, whereas the mortality from the disease among the civilians was as high as 6·5 in every 1000. Again, among the native troops in Ceylon religious prejudices interfere to a large extent with the practice of vaccination. In the year 1819 only four cases (two fatal) of smallpox were observed in a force of 2863 European soldiers, being a ratio of 1·58 cases and 0·8 deaths per 1000 of strength. Among the 4081 native troops, on the other hand, forty-one deaths from smallpox occurred, or 9·8 deaths per 1000 of strength. The general protection afforded to the army and navy, who from their mode of life must frequently have to submit to the chances of contagion, when stationed in large towns, or among unvaccinated populations abroad, may be gathered from the statement of Dr. Balfour, that, taking the aggregate strength of the army at 1,125,840 men, there occurred among them during ten years' service at home and thirty years in the colonies, no more than 745 cases of smallpox and ninety-three deaths, corresponding to a ratio of sixty-six cases and eight deaths per 100,000 men. Sailors, in consequence of the crowding on shipboard, and the necessity of employing natives

* 'Medico-Chirurgical Trans.,' vol. xxxv.

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sometimes in working the ship (particularly on some stations), suffer more from smallpox than the soldiers; but amongst these, during a period of fourteen years, the cases of smallpox have averaged no more than 115, and the deaths ten per 100,000 men. Again, in the Royal Military Asylum at Chelsea, where every boy who has not had smallpox or who does not bear the marks of cow-pox upon him, is vaccinated on admission, during a period of forty-eight years, there occurred in an aggregate strength of 31,705 boys, only thirty-nine cases and four deaths from smallpox, being at the rate per 1000 of 1·231 cases and 0·126 deaths.

Attempts have been made on one or two occasions to estimate and to express by numbers the influence exerted by the contagion of smallpox upon the vaccinated and unvaccinated persons in the population of a district. Thus it is estimated that in Prague, during the twenty-one years extending from 1835 to 1855,—

1 case of smallpox occurred among each	367 $\frac{2}{3}$ of vaccinated persons; while
1 " "	12 $\frac{1}{2}$ of unvaccinated persons.

And again, that—

1 fatal case of smallpox occurred among each	7166 $\frac{1}{3}$ of vaccinated persons; while
1 " "	40 $\frac{2}{3}$ of unvaccinated persons.

Again, during the Marseilles epidemic, 1828,* it is calculated that there were exposed to the influence of the contagious atmosphere, under thirty years of age—

30,000 persons who were vaccinated, of whom 2000, or 6·6 per cent., took the	disease; and
8,000 " unprotected "	4000, or 50·0 " "

Of course, during epidemic seasons, the vaccinated part of the population would be more likely to suffer, still (putting out of the question for the moment the greater mildness of the disease amongst them), Dr. Löschner † calculates that in

* Bousquet, *op. cit.*, p. 195.

† Reply to Mr. Simon's Inquiry. 'Papers,' &c., p. 82.

the district of the Franz Joseph Hospital at Prague, during the epidemic of 1848-9, the disease attacked the vaccinated part of the population in a ratio of 18 per cent., while it attacked the unvaccinated in a ratio of 27·6 per cent.*

Severable unquestionable instances are on record where *the spread of smallpox has been arrested by active exertions in vaccination*. Thus, on one occasion, when smallpox was very prevalent in the parishes round about the town of Exmouth, Mr. Sand informed Dr. Seaton † that he succeeded in keeping it out of the town by vaccinating the whole of the country people between the town and the infected parishes. Mr. Bailey, of Thetford, describes the introduction of smallpox into the village of Caxton, by a parishioner attending the funeral of his mother and bringing her clothes home with him. The daughter, who, from some prejudice, had not been vaccinated, took the disease. Mr. Bailey states that he at once vaccinated three unprotected children at the adjoining house, and ultimately all the children in the village, and there occurred no spread of the disease at that time. Mr. Johnson mentions a village, five miles from Norwich, where smallpox appeared, and where the parents were reluctant to have their children vaccinated. In four or five weeks he succeeded in vaccinating all the children and young persons in the parish, and smallpox was extinguished, although prevailing in all the neighbouring parishes. And Mr. Ceely, of Aylesbury, gives his testimony to the same success in his district. He says that, since 1836, but few cases have occurred in Aylesbury and the adjoining villages. “As soon as a case has been imported from London or elsewhere, prompt vaccination of the inhabitants has soon extinguished it. Twelve cases of small-

* For an abundance of other instances in which well-vaccinated communities have been protected, while badly-vaccinated communities have suffered, see also ‘Sixth Report of Med. Officer of the Privy Council,’ especially pp. 211 and 154.

† ‘Parliamentary Return,’ 1853, p. 59.

pox only existed in Aylesbury during the past winter, from repeated importations from London, Wycombe, and other places adjoining. Extensive vaccination stopped it. In most of the adjoining villages the smallpox was promptly stopped during that period by the same means." Two very interesting accounts, again, of the arrest of the spread of smallpox among the unvaccinated population of India are recorded by Dr. Seaton.* In June, 1850, smallpox broke out along the left bank of the Sutlej. Dr. Cannon immediately set his vaccinators to work along the right bank. The results were, that the disease along the left bank, where there was no attempt made to arrest it, destroyed from fifty to sixty per cent.; along the right bank, from five to six per cent. only, and in many of these cases the proper performance of vaccination was doubtful. The other instance† is that of the arrest of smallpox by active vaccination in two native towns, Chandore and Jooneer. "The inhabitants, numbering several thousand residents," writes Mr. Stuart, the superintendent of vaccination in the Deccan division of the Bombay presidency, "had even up to the period I was appointed vaccinator been much opposed to vaccination, notwithstanding repeated efforts to introduce it among them; at last, variola of a virulent character appearing, petitions were forwarded me to send them a vaccinator. Each member of my establishment thus sent reported to me that, in the majority of cases, the variolous eruption was of the discreet type, but when the people were very densely crowded together it was of the confluent form; and there had been many fatal cases; that there were then above 200 cases in each town, and that it was spreading. The vaccination was speedily established, and increased numbers brought under its effects weekly. After the lapse of three weeks I was informed that the new cases of smallpox were

* 'Parliamentary Return,' 1853, p. 9.

† 'Papers,' &c., p. 141.

diminishing in number, which continued to be the case till the disease disappeared altogether—vaccinia, as it were, advancing, as smallpox receded from the scene.”

That active vaccination is capable of arresting the progress of smallpox cannot be contested in the face of such testimony as this; and it is only a portion of what might be adduced, if space permitted. And yet what is the present state of affairs in London? Notwithstanding our vaccination laws, notwithstanding that the vaccination of young infants in the first three months of life is made compulsory by statute; notwithstanding that there exists for each Poor Law Union in the metropolis a board whose bounden duty it is to carry out to the utmost the provisions of the law; notwithstanding that medical men are appointed to vaccinate gratuitously, smallpox has reigned epidemically in London from 1863 to the very day when I am writing this sentence. And yet I know, and every other medical man knows, and, let me add, that every board of guardians knows (for there is not a health officer in London who has not told them), that, by making a proper effort, an end might be put to such a glaring scandal, such an unutterable disgrace to our civilisation. This is no mere example of Saxon unreadiness. For three years and a half this has been going on, and it is going on still. No wonder the finger of scorn is pointed at local institutions which bring forth such fruit as this. Let us hope that, under other and better control, this at least, among the sanitary necessities of London, will be some day more satisfactorily provided for.

Statistical Argument in favour of Popular Vaccination.

Having thus established by reference to recorded facts the protection against smallpox conferred by vaccination upon individuals and limited communities, I may proceed to inquire

into *the grand results which have followed upon the general practice of vaccination in those countries in which it has been adopted*. For if it be true, and not a mere matter of accident, that vaccinated individuals have by virtue of their vaccination been able to mix with persons suffering from smallpox without taking the disease—that whole districts have been preserved from invasion by this process, while smallpox has been raging all around—that epidemic outbreaks have been arrested by active vaccination, and not merely come to an end, because the force of the outbreak had become expended, in accordance with the ordinary laws which govern epidemics of all kinds—we should certainly expect that so wonderfully potent an influence would, in the course of seventy years, have left its mark upon the history of populations. We will now therefore take up the *statistical argument in favour of the value of vaccination*. It is the argument which found most favour with Mr. Simon. “From individual cases the appeal,” he writes, “is to masses of national experience. Tested by half a century’s trial on the millions of civilised Europe, what has vaccination achieved? Comparing the smallpox mortality of the last forty or fifty years with that of as many years in the last century, do we find any sensible difference?”

Let us see.

In order to state the matter more clearly, it appears to me that it will be well to discuss it under distinct propositions.

First, then, I may say that *the general adoption of the practice of vaccination has, in a very marked manner, reduced the mortality from smallpox in every country in respect of which information upon the subject has been obtainable*.

It is only of comparatively late years that the national importance of national statistics of mortality has been fully recognised; and hence it is that the numerical statements

which I shall have to make of the mortality from smallpox at epochs prior to the commencement of the present century will have to be accepted, not as the precise results of investigations carried on at the time, but as representing roughly a state of affairs, the general truth of which is, however, indisputable. In those times smallpox was the terror of princes and the fierce unconquerable slayer of their people. Cruel and ferocious it still continues to be, but the talons of the monster have been blunted, and it has been confined with a chain so strong and effectual, that none need fear its attack but those who foolishly venture within its range.

To commence, as we naturally should, with *our own country*. It is estimated that, prior to the commencement of this century, out of every million of the population of England, 3000 persons annually fell victims to this disease. This is an average number; in epidemic seasons it would have been greatly larger. During the ten years 1851 to 1860, the mean population of England and Wales* is estimated by the registrar-general at 18,996,916 persons. During these ten years there died of smallpox 42,071 individuals, which is in a ratio of 2214 per million of population for the whole period, or only 221·4 on the average annually. Now, even if we allow the previous estimate (that of Dr. Lettsom) to have been exaggerated, no one can fail to see that a vast difference in the smallpox mortality of the country is observable. And, if it be not exaggerated, one result is that, during the ten years referred to (the latest decennial period respecting which the numbers have been made up), smallpox has been more than ten times less fatal to the people of England than it was when vaccination was not known. During the latter half of the 18th century smallpox inoculation was practised; and there is good reason

* 'Supplement to the Twenty-fifth Annual Report of the Registrar-General,' p. 2 of Tables of Causes of Death.

to believe, as I shall presently show, that while it exercised a protective influence upon those who were submitted to it (the disease thus induced being also milder, for the most part, than that casually contracted), it nevertheless operated perniciously in spreading the disease more widely among the population, by increasing the number of foci of infection.

Statistics have been collected in regard to the mortality from smallpox in London during three several epochs, namely, during the latter half of the 17th century, when inoculation was not practised in the country, during the latter half of the last century, when inoculation was practised, and during the half century ranging from 1810 to 1860, inoculation being mostly disused and vaccination having taken its place. These statistics I have placed in a Table (see Table I, in Appendix). During the first of these epochs, the death-rate from smallpox ranged from 36 to 74 per 1000 of all the deaths that took place, the mean being 56; during the second or *inoculation* epoch, it ranged from 87 to 108 per 1000, the mean being 96; and during the third or *vaccination* epoch, it has ranged from 12 to 42 per 1000, the mean being 25.

Again, from some statistics furnished by Dr. Stark,* it appears that in Edinburgh, during the ten years preceding 1790, the deaths from smallpox constituted, on an average, 131 out of every 1000 deaths that occurred; in the ten years ending 1800, 97 per 1000 of all deaths; while in the subsequent decennial periods, the proportion was as follows:

In ten years ending 1810	...	23 deaths per 1000.
„ „ 1820	...	11·5 „
„ „ 1830	...	13 „
„ „ 1840	...	20·5 „

* 'Parliamentary Return,' 1853, p. 33.

In Glasgow, Mr. Strang estimates the proportion of smallpox deaths as follows :

In the ten years ending 1792	...	350 deaths per 1000 of all deaths.
„ „ 1802	...	318 „ „
„ „ 1812	...	93 „ „

From the numbers which have already been given there can be no difficulty in comprehending how large a proportion of the deaths in any of our centres of population used every year to result from this dreadful malady. It was much the same everywhere. From 5 to 10, or, at times, even nearly 40 per cent. of the deaths that took place, were occasioned by it. And what is the proportion of deaths occasioned by smallpox now? Table 2 (see Appendix) shows what it was during the ten years ending 1850. In Ireland generally it reached, even in 1831-41, 49, and in certain parts even 60 per 1000 of all the deaths; but in most of the towns mentioned the proportion to the deaths from all causes was very much smaller than this. The following Table, in which the gross numbers are taken from the returns of the registrar-general, shows the death-rate from smallpox during the decennial period 1851-60. I have selected the towns on no other principle than that they may represent populations of various character, agricultural, manufacturing, maritime, and mixed; otherwise, they are taken at hazard.

	Mean Population.	Total Deaths.	Deaths from Smallpox.	Mean Annual Smallpox Deaths per Million of Population.	Mean Annual Smallpox Deaths per Thousand of all Deaths.
London	2,583,112	610,473	7150	276·7	11·7
Birmingham	193,286	51,238	774	400·9	15·1
Leeds	109,454	30,345	240	219·2	7·9
Liverpool	263,989	87,898	1007	381·8	11·4
Manchester	236,210	74,359	551	233·2	7·4
Maidstone	37,384	8,468	51	136·4	6·0
Southampton	38,756	9,477	168	433·4	17·7
Luton	27,899	5,662	132	473·0	23·3
Chelmsford	32,518	6,270	9	27·6	1·4
Ipswich	35,320	7,864	112	317·1	14·2
Norwich	71,317	17,774	219	307·0	12·3
Devizes	21,958	4,582	7	31·8	1·5
Plymouth	57,410	13,559	415	792·5	30·6
Falmouth	22,692	4,438	31	136·6	6·9
Wolverhampton	115,530	31,899	473	409·4	14·8
Leicester	64,407	16,366	144	223·5	8·7
Sheffield	116,288	33,084	799	687·0	24·1
York	57,146	13,718	181	316·7	13·1
Newcastle-on-Tyne	100,062	27,388	401	400·8	14·6
Auckland	40,287	9,447	143	354·9	15·1

However high the death-rate from smallpox during the ten years in some of the towns on the above list (and it is seen how greatly it varied), yet in none did it come anything near to that of England at large during the last century, or even to that of London during the latter half of the 17th century.

Passing now from England to *the continent* of Europe, inquiry brings to light a similar series of facts.

The history of smallpox in Denmark, and especially in Copenhagen, furnishes one of the most remarkable and striking illustrations that can be adduced of the influence exerted by the national adoption of the practice of vaccination. Vaccination was introduced into Denmark in the year 1801. During the ten years preceding, there occurred in Copenhagen alone 2546 deaths from smallpox, which, becoming more prevalent in 1801, carried off in that year 486 of the inhabitants of the city. From 1802 to 1810 inclusive, that is, in the course of the nine years succeeding the introduction of vaccination, it occasioned only 158 deaths; and from this latter year until 1824, that is, for the next thirteen years, *not a single death from smallpox* is said to have happened. I have given in the Appendix an abstract of a Table, showing the population and deaths from smallpox in Copenhagen during a century. In 1750, with a population of 60,000, 1457 deaths occurred from smallpox; in 1755, 1117 deaths took place from this disease; in 1759, 1079 deaths occurred; in 1769, with a population of 70,000, 1219 deaths are recorded; whereas although the population had risen to above 100,000 between 1811 and 1823, no death from smallpox happened in all these years. Table 3 is very instructive, and we shall have to recur to it.

According to Mr. Cross,* the first notices of vaccination in Prussia were in 1801, and in 1803 inoculation for smallpox,

* *Op. cit.*, p. 242.

unless under exceptional circumstances, was prohibited. During these three years, it appears that 80,000 persons were vaccinated, and in 1810 it was estimated that the number of persons thus protected amounted to 600,000. Before the introduction of vaccination, 40,000 persons were said to die annually from smallpox within the Prussian dominions. Perhaps this is an over-estimate; 30,000 would probably be nearer the truth. But in 1817, notwithstanding an extension of territory, out of 306,728 deaths only 2940 were occasioned by smallpox. So actively was vaccination carried on in that year, that it is said that the number of vaccinations far exceeded the number of births. There is an interesting statement in Mr. Cross's book relating to the department of Breslau, where in 1818, out of a population of 510,617 persons, 17,639 were successfully vaccinated; and although, during this year, smallpox was introduced at nine different places, only 28 persons took the disease, and only 6 persons died from it.

The same author gives us the following account of events in the principality of Ansprach, in Bavaria:—Vaccination was introduced into the principality in 1801, and in 1807 regulations for public vaccination and the suppression of smallpox were promulgated by the Government, and they were rendered still more stringent in 1808. In this principality, with a population of 266,406 individuals, there perished from smallpox during the years 1797-8-9, on an average, an annual number of 500 persons, and in the single year 1800 no less than 1609 persons. In the year 1807, 500 cases and 50 deaths from smallpox occurred. In this same year, under the new regulations, 37,880 persons were vaccinated, and during this and the subsequent years up to 1818, altogether 141,755 persons underwent the operation. Now, mark what followed. In the year 1808 only 8 cases of smallpox and 1 death occurred; in the year 1809 only 11 cases and 4 deaths; and

from this time to 1818 only 4 cases, every one of which recovered. And yet during these years smallpox was prevailing epidemically in the adjoining kingdom of Wurtemberg, where vaccination was not enjoined until 1818.

In a recent paper by Mr. Hendricks* an elaborate table is furnished, showing the proportion of deaths due to smallpox in Sweden during the latter half of the last and the first half of the present century. The table is too long to be extracted entire. I have made, in place of this, the following analysis, which presents the facts in a striking form. From the year 1749 to 1801 the proportion of deaths from smallpox varied from 1·20 per cent. of the deaths from all causes, in 1786, to 25·45 per cent. in 1779. In these years, *prior to the practice of vaccination*,

The smallpox deaths were under 1 per cent. of all deaths *in no one year*.

„	„	formed 5 and under 10 per cent.	„	in 22 years.
„	„	„ 10 „ 20 „	„	in 17 „
„	„	„ over 20 per cent.	„	in 2 „

Now, contrast with this the mortality from smallpox *during the vaccination period* from 1802 to 1855. In these years the proportion of deaths from smallpox varied from *no death at all*, in 1846, to 3·43 per cent. of the deaths from all causes in 1851:

The smallpox deaths were under 1 per cent. of all deaths in 33 years.

„	formed 1 and under 2 per cent.	„	9 „
„	„ 2 „ 3 „	„	10 „
„	„ 3 „ 4 „	„	2 „
„	„ over 4 per cent. <i>in no one year</i> .		

Viewing all these facts, and especially those exhibited by the table of smallpox mortality in Copenhagen, it surely cannot seriously be argued that the lessened mortality, since

* “Vital Statistics of Sweden, from 1749 to 1855.”—‘Statistical Journal,’ 1862, vol. xxv, p. 142.

the commencement of the present century, is due merely to a lessened severity of the disease from the operation of "progressing civilisation." Did "civilisation," then, make so sudden an advance in or about the year 1801? did all the European nations equally participate in it? Was there, in short, any one thing, capable of being supposed calculated to lessen the ravages of smallpox, which all received in common about that time, except the blessing of the knowledge of Jenner's happy discovery?

A series of tables are to be found in Dr. Seaton's paper,* gathered from particulars supplied to the Epidemiological Society, which Mr. Simon has recalculated, and added to and condensed into one.† I have made an abstract of Mr. Simon's table (Table 4 in Appendix), than which nothing can be more convincing. It shows, for a large number of European states, *the mortality from smallpox* per million of population which prevailed *before and subsequently to the introduction of vaccination*. Some of these contrasts are very remarkable. Thus the smallpox mortality in Trieste during the vaccination period has been 75 times less than it was before, in Moravia 21 times less, in Silesia 29 times less, in Westphalia 25 times less, in Berlin 19 times less, in Sweden 13 times less, in Lower Austria 7 times less, in Copenhagen 11 times less, and so on.‡

In the *second* place it is worthy of remark that *the mortality from smallpox has been least during the vaccination period in those states and places where the most complete arrangements have been in force for bringing the entire population under the protective influence of the vaccine disease*.

On referring to the Appendix, a statement (Table 2) will be

* 'Parliamentary Return,' 1853.

† 'Papers,' &c., p. xxiii.

‡ See also for Prague, Table 5 in Appendix.

found of the proportion which the deaths from smallpox bore to all the deaths that took place in this country and in various centres of population, on the one hand, and in certain continental states on the other, during a period of ten years ending 1850 or 1851. Up to that time, we had no legislation making vaccination compulsory. The Vaccination Act of 1840-41, which was the most advanced statute that had been passed, merely provided for the gratuitous vaccination of all who thought fit to apply to the public vaccinators which each board of guardians was required to appoint. In very many continental countries, however, more or less stringent regulations had been in force for many years. Thus, in Austria, district surgeons are appointed who have nothing else to do but attend to the vaccination of the people; and unless a child be satisfactorily vaccinated, admission into any educational establishment, private or public, is refused; and no parent can receive any charitable relief from the state unless his whole family is protected by having undergone this operation. Similar regulations are in force in Denmark, and have been so from a very early period in the history of vaccination; and in that country, as well as in Bavaria, Frankfort, Russia, &c., no one is permitted to enter upon any business or to marry unless the fact of vaccination be previously established. As early as 1805, in Lubeck, ministers of the Gospel were enjoined to encourage the adoption of the practice, and to recommend it from the pulpit; subsequently various educational disabilities were laid upon unvaccinated children, and public reprobation was invited by the publication of all deaths from smallpox in the newspapers. In France, in Belgium, in Sweden, and elsewhere, a system of rewards is adopted for zeal and activity in vaccinating; and in the last-named country superintendents are appointed to see that all children are vaccinated before arriving at two years of age; and here, as well as in Prussia and Bavaria,

direct fines are inflicted upon obstinate offenders. The result of some of these continental arrangements, by which in most countries vaccination is rendered compulsory, either by direct penalties or by indirect means, is seen in the table referred to. In England and Wales, during eight years, the annual proportion which the deaths from smallpox bore to the total deaths from all causes was nearly 22 in the 1000. On the other hand, in a number of states where vaccination has been more or less compulsory, the proportion of smallpox deaths has not risen higher than from 2 to 8 in the thousand. That a strict enforcement of vaccination may result in a better protection than even now exists in England is also shown by the fact, that whereas during the ten years ending 1860 smallpox carried off annually 221 out of each 1,000,000 of our people, it destroyed annually, during a long period of years, on an average, in Sweden, only 158, in Westphalia only 114, in Trieste only 182, and in Venice, Lombardy, and Dalmatia only from 70 to 87 per 1,000,000 of the population. It is to be observed, too, that in some of the states I have mentioned the pre-vaccinal mortality was nearly equal to or even exceeded our own.

I may take a further illustration of the same fact from an observation made in our own country. It is none the worse because taken quite at hazard. Dr. Seaton, in one of his reports, makes a remark upon the superiority of the vaccination in Yorkshire to that in Wales.* The remark applies, indeed, to the quality of the vaccination; but it is difficult to disconnect this from the general question of efficiency. Now, I had the curiosity to refer to the tables of the Registrar-General, in order to see whether, during the ten years 1851-60, the smallpox mortality was strikingly different in these two districts, and the result is the following, viz.—

* 'Supplement to the Sixth Annual Report of the Med. Officer of the Privy Council,' p. 139.

	Mean Population.	Deaths from all Causes.	Deaths from Smallpox.	Annual Small- pox Deaths per Million of Population.	Annual Small- pox Deaths per Thousand of all Deaths.
Yorkshire .	1,902,294	439,313	4329	227·5	9·9
Wales . .	1,250,874	265,980	4618	369·1	17·3

I do not wish to dwell unfairly upon this comparison, striking as it is; but, having a bearing upon the question, I could not quite pass it over.

In the *third* place it is to be observed that, *where, in the same country or locality, the regulations for the enforcement of public vaccination have been more strict at one period than at another, and where they have been more actively carried out at one time than at another, the mortality of the population from smallpox has receded as the vaccination of the masses has been more perfectly ensured.*

Taking England again as our best illustration, Table 6 in the Appendix represents, for England at large and London in particular, the number of deaths which took place annually on an average of periods of years marked by the existence, in this country, 1st, of no legislation at all relating to vaccination; 2nd, of such legislation as provided gratuitous vaccination for the masses; and 3rd, of legislation which rendered vaccination compulsory. The first Act of Parliament was passed in 1840, and amended in 1841. The Compulsory Act, rendering it penal on the part of parents to neglect the vaccination of their children, was passed in 1853. From this table it appears that in England, during the three years 1838-40, an average annual number of 11,944 deaths from smallpox took place: this was during a period of *no legislation*. During the second period, when *gratuitous vaccination* was provided, that is, from 1841 to

1853, the average annual mortality from smallpox was 5221. During the third period, during which the *Compulsory Vaccination* Act has been in force, that is, from 1854 to 1863, the average annual mortality from smallpox was 3351. Then as respects London. During the three years 1838-40, the mean annual smallpox mortality was 1859: during the 13 years following 826; and during the last 13 years of compulsory vaccination, 758; yet population has rapidly increased. Again, we have seen that during the 8 years ending 1851, in England generally, a period during which vaccination was merely put in the way of all classes without expense, the smallpox deaths constituted about 22 per 1000 of the deaths from all causes. During the 10 years ending 1861, in the 8 last of which vaccination has been compulsory, the deaths from smallpox constituted only 9·9 per 1000 of all the deaths that occurred throughout the country. *An improved legislation was followed immediately by a marked diminution of smallpox mortality.*

Again, the Table 1 in the Appendix is particularly instructive. I allude to that part of it which refers to the several decennials that have elapsed since the discovery of vaccination. During the earlier part of this period, vaccination met with a good deal of opposition: there were old prejudices to be overcome, new convictions to be formed; but, year after year, the national mind became more and more in unison with the teachings of experience. What at first was viewed as a startling innovation, was regarded suspiciously even by the educated public, was occasionally surreptitiously supplanted by variolous inoculation long after the latter was prohibited, as often as opportunity for concealment presented itself, is now in the enjoyment of national confidence. Still the growth of this confidence has been gradual, and its growth is marked by corresponding steps in the recession of smallpox from the metropolis. One de-

cenniad after another has exhibited a diminution in the mortality from this disease—42, 32, 23, 18, 12 per 1000 of all deaths represents for London the lessening ratio in one decenniad after another from 1810 to 1860; and, notwithstanding a comparatively unusual prevalence of smallpox in 1863 and subsequently, the ratio borne by smallpox deaths to deaths from all causes between 1861 and 1865 in London has fallen still further, being represented by $10\frac{1}{2}$. But, during the last ten years, another force has been brought to bear in the metropolis. In 1855 the Act for the better Local Government of the Metropolis was passed; and, in each district of London, the sanitary arrangements were placed under the supervision, and to some degree under the control, of a medical officer of health. Directly, it is true, these officers have nothing to do with public vaccination, but they have, each one I believe, made it part of their business to urge local boards to increased activity, and by friendly communications with the district vaccinators, to direct them to localities where their services were most needed, or where outbreaks of smallpox occurred.

My *fourth* proposition is, that *epidemic visitations of smallpox have been less frequent since the introduction of the practice of vaccination than they were previous to its introduction; and, judging from the mortality they have occasioned, they have also been less severe in their character.*

In Dr. Seaton's letter, which I have frequently quoted already, there occurs a statement* of the years during which smallpox has reigned epidemically in London since 1650. I have extended this statement so as to bring it up to the present date, and added some further particulars.

1. During 91 years, 1650—1740, when *neither variolous inoculation nor vaccination* was practised, there were 65 years

* 'Parliamentary Return,' 1853, p. 26.

in which smallpox prevailed as an epidemic; that is to say, it was epidemic in two thirds of these years.

2. During the 64 years 1741—1803, in which *variolous inoculation* was largely practised, there were 53 years in which it prevailed as an epidemic; that is to say, it was epidemic in six sevenths of these years.

3. During the last 63 years, 1804—1866, in which *vaccination* has been practised, smallpox has been epidemic in London 17 times only, viz., in the years 1805, 1806, 1807, 1812, 1817, 1825, 1838, 1840, 1841, 1844, 1848, 1851, 1852, 1855, 1859, 1863, 1866; that is to say, it was epidemic in about one fourth of these years.

The following is the number of deaths from smallpox registered in London during the more recent of these years :

1841	.	.	Deaths 1053	.	.	Population 1,948,369
1844	.	.	„ 1804			
1848	.	.	„ 1617			
1851	.	.	„ 1066	.	.	„ 2,362,236
1852	.	.	„ 1166			
1855	.	.	„ 1024			
1859	.	.	„ 1156			
1863	.	.	„ 2012	.	.	„ 2,803,989 (1861)
1866	.	.	„ 1388			

With the exception of the severe epidemic in 1863, the mortality has in the latter years been far from commensurate with the increase which had occurred in the population of the metropolis.

The return made to Mr. Simon's inquiries from the kingdom of Denmark afford another remarkable illustration of this influence. Referring to Table 3, we see that during the fifty-two years from 1750 to 1801 when vaccination was first introduced, there were four years in which smallpox was so diffused among the population of Copenhagen as to occasion a number of deaths exceeding 1000, and nine years in which

it occasioned above 300 deaths. Hence we may gather that, putting aside the fact that the disease was always more or less present in the city, there were thirteen years of more or less remarkable epidemic prevalence—that is to say, it was epidemic on an average in *one year out of every four*. On the other hand, during the forty-nine years (from 1802 to 1850) in which *vaccination* has been practised, there have *only been two years* in which an epidemic of any consequence occurred, namely, in 1835, when, notwithstanding the increased population, only 434 deaths from smallpox took place, and the year 1843, when only 111 deaths occurred.

Again, we are told that in Iceland, from 1241 to the end of the eighteenth century, a record exists of nineteen severe epidemics. Since the introduction of vaccination only one epidemic has occurred, namely, in 1839, and this was very mild in its character.

I repeat that I am here estimating the mildness or severity of epidemics by the general mortality they have occasioned among a mass of people. Regarded from another point of view, *smallpox among the unvaccinated is as cruel an enemy as ever it was*. From time to time it varies, it is true, as it ever did, in the character it assumes; sometimes mild, sometimes malignant, or hæmorrhagic, as in the Geneva epidemic described by Marc d’Espine.* But, taking epidemics as they affect all classes, vaccinated and unvaccinated, experience assures us that “*epidemics of smallpox have been,*” as Prof. Hebra† believes on the whole, “*more rare and less malignant since the introduction of vaccination.*”

Similar to European experience is that of India. Smallpox, it appears, is in the habit of visiting India once in three years: at each visit about two thirds of those capable of receiving it are attacked, and of these nearly one half die.

* ‘Archives Générales,’ fifth series, t. xiii et xiv.

† ‘Papers,’ &c., p. 134.

Mr. Don,* writing to Dr. Seaton, says, "Since vaccination was introduced in 1812 into the Purgunnah of Broach, smallpox has twice appeared; once very fatally; but it made very little progress in the vaccinated villages, and on inquiry, in 1817, not one of the vaccinated was found to have been attacked, although about 7000 were inspected. At Keira, vaccination was introduced in 1813, and there had been no epidemic for many years; at last it appeared in 1824, and only attacked the unvaccinated, at least there was no fatal case among the vaccinated. At Ahmedabad, vaccination was introduced in 1817, became general in 1825, and since then smallpox has not been heard of."

There are two other evidences of the influence which the popular adoption of vaccination has had, which, although less precise in their character than those I have adduced, are nevertheless not to be lightly passed over; they are patent to everybody who is old enough to recollect what he was in the habit of seeing in society and in the streets some thirty or forty years back. I allude to the number of persons one was in the habit of meeting whose *faces were scarred and seamed by a previous attack of smallpox*. How rarely is this observed now! Even among the lower classes of the community it is not common, and among the upper and middle classes it is scarcely ever to be observed. Another of the results of smallpox which has become rare is *blindness*. Lévy† tells us on good authority that, before the time of Jenner, 35 per cent. of all the cases of blindness met with were due to an attack of smallpox. Now, however, the proportion of cases arising from this cause forms only 8 per cent. of all cases. Out of 100 blind children, M. G. Dumont found only three whose blindness was due to smallpox, and

* 'Parliamentary Return,' 1853, p. 9.

† 'Traité d'Hygiène,' t. ii, p. 482.

the same observer estimates the reduction in the number of blind people in France, due to the practice of vaccination, as not less than one fourth.

Is there any reason to believe that the practice of vaccination, while lessening the ravages of smallpox, has had the effect of promoting the occurrence of other fatal maladies?

Detractors of vaccination have not contented themselves with denying the truth of the general proposition I have, in the preceding pages, been engaged in establishing, nor yet with warping statistical facts until they fit the opinions which they have thought proper to adopt; but they have brought distinct and sweeping charges against the practice which, if true, would indeed cause every thinking man to hesitate before he awarded it his concurrence or advocacy. I may take M. Eymard, of Grenoble, quoted by Bousquet, and M. Carnot, of Autun, as representatives of this class of maligners. The former of these gentlemen appeals to the fact that, in the town of Grenoble, population had not increased for twenty-five years after the introduction of vaccination into it; and argues from this that, although the practice of vaccination has operated as a preservation from smallpox, such preservation has only served to replace one disease by another in such a manner as to re-establish the disturbed equilibrium. It is difficult to treat an objection raised on such a ground as this with becoming seriousness; but I cannot well pass it over, because it is an example of a kind of argument much in vogue with illogical minds. The facts are plain. Vaccination was introduced at a certain time, has been in use for so many years, and, at the end of those years, the population, despite of the births that have taken place, is no wise larger than it was at the commencement of

the period : *ergo*, the vaccination has had the effect of checking the natural increase of population. M. Eymard seems to have forgotten that the amount of a population, its increase or its decrease, depends upon a variety of other causes besides the operation of disease or that of conditions affecting health. Its trade may have fallen off, or have been diverted to other quarters, or its people may have seen other good reasons for unusually frequent or extensive emigration ; while the inducements for others to supply their place in the town may not have been such as to lead to a restoration or increase of number. Besides, Grenoble is not a town, after all, of the first magnitude, nor is it the only place where vaccination had been practised. If the result of the practice be really such as he suggested, it would be found more distinctly still where large masses of population are concerned ; and throughout Europe generally we should discover that the increase of the people had been arrested. But it is notorious that this is the very reverse of fact.

M. Carnot enters into a more elaborate statement of the evil results of the practice of vaccination. He expresses an opinion that, during the present century, a displacement of mortality, as regards the age at death, has taken place ; that the age at which death takes place is only later than it used to be, and that thus the chances of attaining forty-one years of life are the same now, for an infant, as they were last century ; that while smallpox, measles, and other diseases of infancy have lessened, fever, cholera, dysentery, and other diseases to which adult life is prone, have increased ; that, in France, the births are so much diminished as to be less numerous than the deaths ; so that, like Eymard, he dreads a general depopulation, and he especially dwells upon typhoid fever as the grand agent of destruction.

It would be impossible for me to refute these views of M. Carnot in a more able and satisfactory manner than they

have already been refuted by Mr. Simon.* I shall, therefore, content myself, almost solely, with presenting, in as few words as possible, the facts and reasonings adopted by that author. First, then, he shows, with respect to diseases generally, that while, under the influence of vaccination, smallpox has been diminishing its ravages, so, under other influences, have other diseases been diminishing theirs; and he appeals in evidence to national statistics.

According to the calculations of Dr. Farr, the average annual death-rate per 10,000 living in London, from all causes and at all ages, in seven different periods of time during more than two centuries, may be stated as follows :

Period.	Death-rate.
A. 1629 . . . 35	500
B. 1660 . . . 79	800
C. 1728 . . . 57	520
D. 1771 . . . 80	500
E. 1801 . . . 10	292
F. 1831 . . . 5	320
G. 1846 . . . 54	248 $\frac{9}{10}$

Since 1854, the London death-rate has been smaller than even the last of these figures. "The first line (A) shows, for the period 1629-35 (though almost exempt from epidemic disease), a general death-rate just double our present one; in the second line (B), it is seen that, for the twenty years 1660-79 (including the fatal one 1665), the death-rate was three and one fifth times as great as it now is; and in the fourth line (D), it is shown that during ten years (1771-80), towards the end of the last century, when smallpox was fourteen or fifteen times as fatal as now, the general death-rate was still double."

Mr. Simon quotes the statistics of Copenhagen, furnished him by the Danish Government, to show that they correspond with the statistics of London. Thus, from 1750 to 1775,

* 'Papers,' &c., p. xlvii.

there was, on an average, an excess of deaths over births to the extent of 853 annually; from the last date to 1815, there was an excess of deaths over births, but only to the average annual extent of 82; while in the years 1816 to 1850, the *births exceeded the deaths* to the extent of 304 annually. Of the twenty-six years in the first period, there were twenty-three in which the deaths exceeded the births; of the forty years of the second period, there were twenty-five in which the deaths exceeded the births; while in the thirty-five years of the third period, there were but six years in which such an event occurred. The average annual death-rate in Sweden at four periods, ranging from the middle of the last century until the middle of the present century, was as follows.

Date.	Death-rate per 10,000.
1755 . . . 75	289
1776 . . . 95	268
1821 . . . 40	333
1841 . . . 50	205

The next point taken up by Mr. Simon is Carnot's assertion with respect to the threatened depopulation in France. Taking the calculation of M. Bertillon, whose conclusions were unreservedly adopted by the French Academy of Medicine, he says, "A very slow increase, or possibly a decrease of the French population at the present time, seems, indeed, to be an admitted fact; and it is stated (I believe on the authority of official documents) that the standard of height for admission into the French army has of late years of necessity been reduced, because of the decreased stature of the general population; while, nevertheless, the proportion of conscripts found physically incapable of service has undergone a continuous increase. Be it so. . . . Would any reasonable person proceed from these particulars to construct a universal theory (the first

deduction from which must be that such particulars are general in Europe), never verifying his theory by any second instance, never looking for those imputed effects of that same cause in other lands where it operates? Should it not be a first impulse to ask, are these things so elsewhere? Do other countries suffer like this pitiable image of France? Is England beginning to be depopulated? are its women becoming less fruitful? Does Sweden show a *déplacement de la mortalité*? is its adult life now more precarious than fifty years back? In Geneva, where mortuary records have been kept for three centuries, are any such results reported? Is the revaccinated army of Prussia wasting away with a quadrupled mortality? Does Bavaria, among its conscripts for military service, show an increased proportion of incapables? The most cursory examination of this kind might have convinced M. Carnot that, whether his arithmetic be right or wrong, his medical conclusions are untenable."

It is well known how steadily each successive decennial census shows an increase of population among the millions of our people, and that the fecundity of our women is not falling off is shown by the fact that, while in the years 1838-40, for every 1000 women aged between 15 and 45 there were registered $133\frac{1}{3}$ living births; in the period 1851-60, for every 1000 women of like ages, there were $144\frac{1}{3}$ registered.

Mr. Simon quotes a table prepared by Dr. Farr, showing the annual death-rate at various ages in Sweden (Table 7, in Appendix), at four different periods, namely, during twenty-one years 1755-75; twenty years 1776-95; twenty years 1821-40, and ten years 1841-50. This table shows that "in the penultimate period (within which the fatal cholera epidemic of 1834 killed 12,637 persons), the population at all ages under 30 years of age, consisting, of course, for the most part, of vaccinated persons, showed a much less death-rate than the population of the like ages in the former

century. And in the next period (1841-50), when vaccination would have affected, at least, all ages up to that of forty years, the corresponding death-rates show an improvement on the earlier vicenniad 1821-40, and a still more striking improvement on the death-rates of the last century."

The two following tables, the one embodying the conclusions from some researches of Marc d'Espine as respects the city and canton of Geneva, and the other having reference to London, indicate that the displacement of mortality is certainly of a character not to cause alarm, inasmuch as it is displaced to such an extent as to throw it from the younger ages far beyond the period of middle life; in fact, that the displacement is just such as it is the object of all sanitary efforts to bring about.

Survivance in Geneva at various periods, from 1560 to 1843.	Percentage of those born who reach 10 years of age.	Percentage of those living at 10 years of age who survive to 40.
CITY OF GENEVA.		
1560—1600	42	43
1601—1700	48	53
1701—1760	60	68
1761—1800	61	71
1801—1813	69	72
1814—1833	74	72
CITY AND SUBURBS.		
1816—1830	74	74
CANTON.		
1838—1843	74	71

Composition of 1000 Deaths in London at two different periods.

Ages.	Bills of Mortality, 1738—43.	Registration of City of London, 1848—55.
0— 5	455	375
5—10	36	42
10—20	31	42
20—30	76	63
30—40	91	80
40—50	93	90
50—60	82	92
60—70	62	100
over 70	74	116

But in addition, specific charges have been brought against vaccination to the effect that, since its introduction, *typhoid fever* and *scrofulous affections*, including phthisis, have committed greater ravages than they did previously, the inference of course being that we may thank the practice of vaccination for the difference. At the request of Mr. Simon, Dr. Greenhow* took a great deal of pains to investigate statistically the truth of this statement, a task of no small difficulty, in consequence of the changes which have taken place in the nomenclature of diseases, and the imperfections of the earlier records. The result is that *fever* (including a variety of other diseases formerly included under that term), which now produces a death rate of 385 per 100,000, a century ago gave a death rate of 539 per 100,000: and Dr. Farr is also quoted as stating that “fever has progressively subsided since 1771, and that the combined mortality of smallpox, measles, and scarlatina is now only half as great as the mortality formerly occasioned by smallpox alone.” French observers have also established, in opposition to the dictum of M. Carnot, that smallpox and typhoid have no discoverable etiological relation

* ‘Papers,’ &c., p. 27.

the one to the other, susceptibility to the infection of the one having nothing whatever to do with susceptibility to that of the other.* Besides, Mr. Simon most justly observes "that the investigations made in this country have established among the certainties of medicine that typhoid fever mainly depends on causes quite remote from the causes of smallpox." As regards *scrofulous and tuberculous affections*, Dr. Greenhow's statistics are as convincing as in the instance of fever. "Exclude phthisis from the comparison (because of the formerly imperfect means of recognising its presence), and the scrofulous death rate per 100,000, which in 1681-90 was 801, and in 1746-55 was nearly 1099, is now but 206: so that, looking to the middle of the last century, the golden age of the vaccino-phobists, we find a *scrofulous death rate more than five times as great as our present one*. And then, trying by a different process to estimate the former fatality of phthisis,—examining, namely, for the three periods compared, what deaths have been attributed to disease of the respiratory organs—we find that, even with the utmost amplification of this list (including pneumonia, which formerly may have been counted to fever, and including respiratory diseases of infancy, which would formerly have been counted to "chrisomes," and including similar affections of advanced age, which would formerly have been counted to "old age"), still *the pulmonary death rate of the present time is seven per cent. lower than the pulmonary death rate of 1746-55*. Dr. Farr's conclusion quite confirms the tendency of Dr. Greenhow's evidence, and he remarks, as the general result of his inquiry, that 'the proportion of persons destroyed by consumption with other forms of scrofula has (except in the anomalous period, 1771-80) progressively declined in Lon-

* It is quite needless to reproduce the mass of evidence on this point which will be found in Mr. Simon's report, p. 57. It is only for the sake of completeness that I discuss this point at all.

don." And neither does what we know positively of the etiology of scrofula and tuberculous diseases lend any aid to the opinion of those who regard them as in any way resulting from the practice of vaccination. The condition of the system in which what are commonly termed scrofulous affections arise is one of debility and malnutrition, the concomitant of poverty, damp, darkness, and a variety of other unhealthy surroundings, amid which children often have to grow up and even labour for their bare living. Surely smallpox must weaken more than vaccination those whom it does not kill. The killed by smallpox we have learned something about, the wounded may be estimated from the vaccination reports of the French Academy. Thus I find on referring to these for four years that—

In 1861 out of 9678 cases, 1746, or 1 in $5\frac{1}{2}$ died, and that 829, or 1 in $9\frac{1}{2}$ of those who recovered were disfigured or rendered infirm.

In 1862 out of 13,375 cases, 1813, or 1 in 8 died, and that 1263, or 1 in 9 of those who recovered were disfigured or rendered infirm.

In 1863 out of 13,188 cases, 1440, or 1 in 9 died, and that 1199, or 1 in 9 or 10 of those who recovered were disfigured or rendered infirm.

In 1864 out of 29,576 cases, 3290, or 1 in 9 died, and that 2231, or 1 in 11 of those who recovered were disfigured or rendered infirm.

Can anything of this kind be shown among the effects of vaccination? And as respects those forms of constitutional disease which are actually tuberculous, putting aside the well-known association of them with hereditary tendencies, it is an established pathological truth that among the exciting causes of tuberculous alterations of nutrition impoverishing and depressing causes hold the most considerable place. It is only as a debilitating cause that either smallpox or vaccination can be imagined as operating; and while all writers attest the frequency with which scrofulous affections follow in the train of smallpox, so, as vaccination is less depressing, in just such measure does its substitution for smallpox act in prevention of scrofula. Such, with some

trifling addition, is the line of argument adopted by Mr. Simon. I need only say further that Dr. Quain found as the result of a special inquiry in respect of phthisis, that thirty per cent. or more of the patients in a public hospital suffering from that disease showed no marks of vaccination whatever. Such an observation as this tells powerfully in favour of vaccination, since the proportion of unvaccinated persons to the whole community in London which avails itself of cleemosynary institutions is, judging by the number of unvaccinated children in public schools, not much above six per cent.

I may conclude by quoting the following passage from Bousquet :* "I protest strongly," he says, "against a system which only accords to vaccination the miserable advantage of substituting one evil for another. It does nothing of the kind. In fact, is it not obvious to any man that, on this supposition, death would find its victims principally in the classes of society which are in the most easy circumstances, for the reason that, amongst these vaccination is most completely carried into effect? The truth is the very opposite to this, it is the poor whose mortality is greatest."

Is Vaccination in itself a dangerous Operation?

Another objection has been raised to the practice of vaccination, which it appears to me that this is the proper place to consider. It has been said that the operation is in itself one not free from danger, and even from danger to life; and instances are put forward in which severe erysipelas, blood-poisoning, or ulceration have followed upon its performance. It has been said that it is especially dangerous in the case of very young children; and a few years ago the same outcry was raised against the practice of re-vaccination, public attention having been especially directed to the subject in consequence of the lamented death of Sir Culling Eardley.

* 'Traité de Vaccine,' p. 131.

I am not going to deny that occasionally severe effects, and even a fatal result, have followed the introduction of healthy vaccine virus into the system ; that sometimes it has happened that instead of producing, as it normally does, a local vaccine vesicle, the system appears to be affected by it, as it would be by the introduction of some virulent animal poison ; or that, in other instances, after the normal operation is completed, the system or the punctured part exhibits the results of an impression very much deeper and different to those usually met with. Most, if not all, of these unfortunate cases are open to an explanation, however, based upon the peculiarities of individual constitution, upon some condition of ill health at the time of vaccination, upon the mode of life pursued during the progress of the vaccine disease, or upon some circumstance such as is known to impart a severe character or a fatal tendency to other affections. It was said, for instance, in Sir Culling Eardley's case that his health at the time of the operation was not in a completely satisfactory condition ; and in another case recorded by Mr. Wells, where death followed revaccination, that, on a previous vaccination, an unusual idiosyncrasy was manifested by the occurrence of a prostration almost bordering upon death. There is, I believe, sufficient evidence from general experience, that the constitution of adults reacts more fully and decidedly under the stimulus of the vaccine virus than that of children, for in them we more frequently meet with rigors, anorexia, headache, painful enlargements of the axillary glands, extensive erysipelatous redness of the arm and shoulder, and other manifestations of "general indisposition." At other times the development of erysipelas after vaccination may be traced to the operation of the same cause which occasions this affection after surgical operations of any kind. Thus we find it related* that in October, 1859, erysipelas

* 'Army Reports for 1859,' p. 24.

followed revaccination in four cases at Parkhurst in the Isle of Wight; and in one of these cases sloughing occurred to such an extent as to render necessary amputation at the shoulder-joint. At that time, however, there was a general cause of ill health in operation, for both typhoid fever and erysipelas were prevailing at Newport, and several cases had occurred at the barracks. Under such circumstances all surgical operations, even the most trifling, are dangerous, as every surgeon knows. A parallel event, but on a much larger scale, happened in the French army in 1858.* On the 29th June in that year several men were admitted into the hospital with engorgement of the axillary glands, phlegmonous erysipelas of the arm, &c., as the result of a vaccination effected on the 21st. Sixty men were vaccinated with every precaution and unexceptionable lymph. The next day one of them was attacked with phlegmonous inflammation of the arm, complicated with typhoid fever, the origin of which was probably anterior to the vaccination. Three days later another soldier was similarly attacked, and the fourth day seven more, but without the typhoid. M. Larrey, who was sent to inquire into the affair, noted amongst its causes first of all a very high atmospheric temperature, and, secondly, an unfavorable medical constitution. Erysipelas was prevalent in the town of Toulouse, where this accident happened, as well as in the hospital, and the men had, moreover, undergone an unusual amount of fatigue on account of an approaching inspection of the troops. It is evident that the mere vaccination was not in fault, since, during four years, more than 12,000 soldiers forming the garrison of Paris had been revaccinated without any serious accident occurring.

Dr. Lyman† has collected into a table a large number of instances in which erysipelas or troublesome ulceration

* 'Rapport sur les Vaccinations en France pour 1860,' p. 21.

† 'American Medical Times,' vol. iv, p. 134.

have followed vaccination in the practice of several British and American surgeons. The greater number were in children, but then it is to be recollected that vaccination is mostly performed upon children, and hence the fact tells us nothing of their superior liability to such accidents. Of the six adults, two were between sixty and seventy years of age; one was described as an "old man;" one was thirty; one was twenty-five, and the sixth eighteen years old. Seven of the cases proved fatal, being attacked with erysipelas on the ninth day in three instances; on the tenth in one; on the eighth in one; on the seventh in one; and in one case the time of attack was not specified. In some of the other cases it occurred on the second, third, or fourth days. The season of the year in which these cases occurred is deserving of notice. Excluding some cases of tendency to troublesome ulceration during the prevalence of a cold north-east wind in the month of May, only three of the whole twelve cases or groups of cases collected were observed during the warmer months of the year. With regard to the cases in the month of May just mentioned, it is stated that the same tendency to troublesome ulceration declared itself also in persons who were at the same time inoculated for small-pox. With regard to a group of eight cases of erysipelas occurring in the practice of six several physicians at Boston, U.S., in the winter of 1849-50, it is stated that that winter and spring were distinguished by a tendency in other cases besides vaccination to the occurrence of the erysipelatous form of inflammation. In one case of erysipelas followed by sloughing and necessitating amputation, related by Mr. Chartres, it is noted again that an epidemic tendency to unhealthy inflammation was observed in all cases then in the hospital, so that it became necessary to avoid every surgical operation; and even the punishment of the soldiers by flogging was suspended on account of the erysipelatous inflammation

which occurred whenever the cutaneous surface was injured. At such seasons as these the practice of M. Musart is well worthy of imitation. He says,* “Whenever in the course of my vaccinations I have perceived a tendency to epidemic erysipelas, as in 1861-2, I have given up vaccinating and not taken to it again for more than a month.”

Sometimes, however, the occurrence of erysipelas and its results may be the fault of the operator, who uses lymph taken from a pock too far advanced, or lymph which has been preserved in the fluid state so long or under such circumstances as to permit of its decomposition, or who may have taken it from a subject recently blistered. Thus an instance is recorded† of nine persons vaccinated in the year 1800 with virus of a purulent appearance taken from a vesicle at a very late period of its course; extensive erysipelas resulted, followed by ulceration, and, in some of the instances, by a tendency to gangrene. Of a large number of persons vaccinated at the same time but with different lymph, none suffered. Mr. Wakley‡ mentions the case of two infants aged respectively two and six months, vaccinated with good lymph (originally) preserved in a bottle for thirty or forty hours, and which in that time had undergone decomposition. In both infants the arm became greatly inflamed; one of the children died with sloughing of the wound, and the other had abscesses form in the joints and elsewhere, but recovered. Dr. Lyman§ also mentions an instance where fifty persons were vaccinated with a fluid produced by dissolving otherwise good scabs in water, the *phial having been carried about in the pocket of the operator*. All these persons had very severe erysipelatous and gangrenous inflammation supervene,

* ‘Rapport sur les Vaccinations en France pour 1863,’ p. 90.

† ‘London Med. and Phys. Journal,’ vol. iv, p. 488.

‡ ‘Lancet,’ July 10th, 1852.

§ ‘American Med. Times,’ vol. iv, p. 107.

and three of them died. With respect to the danger of taking lymph from persons recently blistered with cantharides, Mr. Ceely* states that he has known erythema and erysipelas propagated from "irritable," late, ruptured, or exhausted vesicles, or from apparently healthy ones on a child who had a blister behind the ear. Dr. Hudert† relates an instance of five children all vaccinated from the same child, in whom very quickly the arms became swollen and œdematous, one child suffering from convulsions and two from abscesses, no vaccine vesicle rising on any of them. The child from whom the lymph was taken had, the evening before, had a blister applied behind the ear. Four of these children were vaccinated with clean lancets, and the fifth, living at a distance from the other, with points.

Husson‡ relates also the history of two persons in whom sloughing ulcers resulted from the application of vaccine lymph to a surface blistered with cantharides. One can scarcely wonder, then, at the occasional occurrence of accidents, where proper precautions are not taken in all these respects. Drs. Seaton§ and Buchanan positively tell us of three public vaccinators who did not hesitate to make use of moist lymph, preserved in a bottle, *after it had been kept for a week*. "Indeed, one vaccinator expressed his opinion that the lymph being ammoniacal and putrid, was no obstacle to its success, nor to the regular course of the vaccine disease that was produced by its insertion !"

The question whether vaccination is associated with *especial danger in the earliest periods of infancy* is one which demands a few separate remarks. Very opposite opinions have been put forth upon this subject, which is fully dis-

* 'Papers,' &c., p. 125.

† 'Med. Gazette,' vol. xiii, p. 440.

‡ 'Dict. des Sciences Méd.,' t. lvi, p. 423.

§ 'Sixth Report of Med. Officers of the Privy Council,' p. 102.

cussed in the 'Report on Vaccination in France for 1860.' M. Husson,* whose experience in vaccination was very extensive, states that he vaccinated newly-born infants without any bad results. He held the opinion, that the younger the subject vaccinated, the less he suffers under the development of the disease, a privilege Husson regarded as due to the suppleness of the skin and the more ready yielding to extension on the part of the cellular tissue. Bousquet, however, held that the cutaneous excitation was apt to show itself in the form of erysipelas or roseola, &c., and, in infants very young indeed, that the intestines sympathised, and that enteritis or diarrhœa might result. Barthez met with two cases in which infants, vaccinated on the second day from birth, suffered severely from the effects of the operation, and one of them died; still he hesitated to regard these as anything but exceptional events. He communicated the facts to the Société Médicale des Hôpitaux, when M. Legroux stated that he had been in the habit of vaccinating newly-born infants, and had become convinced that the number of punctures had much to do with the occasional accidents that followed; that, since he had reduced the number of punctures to one on each arm, he had not met with the least accident. M. Behier said that, although in the constant habit of vaccinating infants on the second day, he had never met with any evil results, whereas he had seen erysipelas attack and prove fatal to infants who had never been vaccinated at all. M. Ragaine states that he has vaccinated some 400 infants, from eight days to a month old, and has always observed the disease run a mild course, having never seen roseola, or erysipelas, or enteritis follow the operation. M. Laforgue says that, at the Hotel Dieu at Toulouse, it is a practice to vaccinate in the first days from birth, and that his experience has demonstrated the com-

* 'Dict. des Sciences Méd.,' t. lvi, pp. 378, 379.

plete harmlessness of the operation; but that on one occasion, when some observations were being made with a new virus from the cow, one infant, aged 14 days, suffered from erysipelas, extending over the whole of the body, but nevertheless recovered. Indeed, his observation at the Maternité of Toulouse was to the effect that such very young infants, as a general rule, suffered less from the inflammatory reaction than other children who were older. His opinion is that, when accidents do happen, they are due to causes foreign to the vaccine disease. M. Godefroy* states, in 1861, that for many years at Rennes infants had been vaccinated at ages from one hour to seven days by means of three punctures in each arm, and he had never seen erysipelas or phlegmon, or any other accident, result from the practice. M. Jobert transmitted to the French Academy his experience upon this subject during a period of thirty years of practice, in which he had performed 20,000 vaccinations. He found that engorgements of the axillary glands, phlegmons, and erysipelas, were accidents not more frequent in the earliest periods of life than when the vaccination was deferred to some months or some years later. Out of 1200 infants vaccinated at periods of age varying from 1 to 30 days, he met with erysipelas once in 300 cases, phlegmon once in 477 cases, and sub-axillary engorgement once in 300 cases. He makes the remark, that these accidents were rather more frequent, indeed, when infants were vaccinated after they were three months old. In such, he met with erysipelas once in 295 cases, phlegmons once in 420 cases, and sub-axillary engorgement once in 250 cases. Again, M. Danyan observed at the Paris Maternité, out of 200 infants vaccinated in the first days of birth, only three accidents altogether; once a phlegmon, which got well, and two cases of erysipelas of the arm, one of which was fatal. The writers of the Report from which I am quoting,

* 'Gaz. des Hopitaux,' 1861, p. 466.

while confirming, from their own extensive observation, the general innocuousness of these very early vaccinations, do not deny, as indeed they cannot, that accidents sometimes happen: they do so after other operations, however simple they may be. But they urge, and I think fairly, that these and such like accidents are common to all ages, and, in infants, are often due, not directly to the vaccination, but to the condition and circumstances of the children, their defective nourishment, and the insalubrious localities in which they often live.

The following case finds its interest in the fact that the fatal result was apparently due to the high susceptibility of the child to the operation of the vaccine virus, which produced a disease much more nearly resembling what we see in other exanthematous maladies, and especially an impression upon the nervous system which nothing will account for but unnaturally exalted impressibility.* A ricketty child was vaccinated on April 30th successfully. On the ninth day the vesicles were large and filled out, and there was a papular rash all over the body. On this day fits of eclampsia appeared, which lasted into the next day, when the child died, apparently from extension of the spasm to the respiratory muscles. The child was two years old, and had cut all its teeth except the canines. Dr. Drysdale attributes the eclampsia "to the presence of the vaccine poison in the blood;" I would rather say that the child died in the stage of the vaccine disease corresponding to the eruptive stage of inoculated smallpox, when the impression upon the nervous system is most powerfully made.

But, after all, what do all these accidents amount to? Is there any disease of the class to which vaccinia belongs, is there any the most trifling operation that the surgeon can

* Dr. Drysdale, 'Med. Times and Gazette,' 1863, vol. i, p. 545. I have already hinted that the general eruption might have been one of smallpox.

perform that is not open to an unfavorable termination such as is met with now and then in vaccination? And from like causes. Are we to throw aside a protection like this, because we cannot ensure absolute and certain immunity from evil consequences? Should we act in this way—do we act in this way—when other remedial and prophylactic advantages are to be gained? do we hesitate to administer powerful drugs because one person out of a hundred or two is so constituted as to suffer inordinately from their operation? do we deny the soothing and beneficial action of chloroform inhalation because now and then a fatal result has happened from its employment? do we not act upon the teachings of general experience, in place of accepting as our guide occurrences which are altogether exceptional? Can anything better be proposed? Is it to abandon vaccination? Is, then, smallpox a less dangerous disease?

The lesson which these accidents impart to us is this: that vaccination is not a thing to be trifled with, or to be made light of; it is not to be undertaken thoughtlessly or without due consideration of the condition of the patient, his mode of life, and the circumstances of season and of place. Surgeon and patient should both carry in their minds the regulating thought that the one is engaged in communicating, the other in receiving into his system a *real* disease—as truly a disease as smallpox or measles—a disease which, mild and gentle as its progress may usually be, yet nevertheless now and then, like every other exanthematous malady, asserts its character by an unusual exhibition of virulence. The person who bears a vaccine pock upon his arm, should, during the course of the affection, at any rate, act as if he believed this, and, for the time being, avoid, as a sick person would, all those excitements which at other times he may indulge in with impunity, and should do nothing which is calcu-

lated to add to the disturbance which the evolution of a morbid virus naturally produces in the constitution or in the blood.

THE PROTECTION AFFORDED AGAINST SMALLPOX BY VACCINATION
IS NEITHER UNCONDITIONAL NOR CONSTANTLY UNLIMITED ;
BUT MANY OF THE CONDITIONS UPON WHICH IT DEPENDS
ARE UNDER THE CONTROL OF MANKIND.

I think, after what has been said, it must be admitted that the practice of vaccination has established for itself, since the commencement of the present century, a claim to public confidence, which it would now be a hopeless task to dispute. It founds this claim on the individual protection which it affords to those who have recently undergone the operation, and even for years afterwards, both against the inoculated virus of smallpox and the casual exposure to infection ; upon the fact that, in epidemic seasons, those who have undergone the operation are vastly less liable to attack than those who have not undergone it ; that limited communities of vaccinated persons have remained scatheless in the midst of a desolating epidemic ; that history supplies us with examples of the arrest in the march of smallpox through a country or a town, or of a defensive barrier against its entrance, when threatened, being set up, by an active vaccination of the inhabitants ; upon the fact that, wherever vaccination has been adopted as a national practice, smallpox has ceased to be the dread and the destroyer that it used to be ; that the more perfect the popular vaccination is, the more complete is the protection it has afforded ; that, during the last half century, epidemics of the disease have been less frequent visitants than formerly, and less fatal ; and finally that, while

the disfigurement which was once so common is now comparatively rare, one of the most common causes of blindness, the saddest of all afflictions—with one exception—which can befall a human creature, has been almost done away with. Can it occur to any reasonable person that it would be well to recur to the days when vaccination was unknown; that the world would be the better and the happier, that men would be more robust to labour, and women more beautiful to charm, or that fewer parents would mourn over the early grave of their children? It is strange, but such persons do exist. The erratic paths discovered and pursued by some minds baffle comprehension.

Up to the present time I have been engaged in showing the general protective influence exerted by vaccination. It now becomes necessary to discuss the question more precisely; inasmuch as it is a matter of public notoriety that smallpox does occur even among those who have undergone the vaccine disease. That it does so is indeed one of the most important of the objections raised against the protective power of the operation. I will state the fact presently as broadly and as candidly as I can, because, in a discussion where truth alone has to be elicited, there can be and ought to be nothing to be concealed; and further, because it will be my business to show how and why it is that the protection afforded is not always so complete as the public are apt to anticipate. But, before I do so, I would say to some of those who are ready to support their assertion that the protective power of the operation is a delusion by adducing instances from their own experience, that there are two fallacies which sometimes vitiate their supposed facts, and consequently the conclusions they arrive at. One of these is that many persons who are reputed to have been vaccinated have, in truth, never passed through the vaccine disease at all. Among the upper classes of society this fallacy is less

likely to operate, because the disease is watched carefully through its course by the medical attendant of the family and, if the operation should fail on the first attempt, he is pretty certain to make the friends of the child aware that it is not protected, and to make a farther trial at a subsequent period. Amongst the lower classes, however, it is not so; they mostly are required to visit the public vaccinator on the eighth day from vaccination, in order that he may form for himself an opinion as to the success. From various causes, it commonly happens that parents neglect to obey this injunction; either they do not see the reason of it, or they fancy that the operation having been performed the magic rite is accomplished, or they see that some effect has been produced, and are consequently satisfied that the result has been successful. The other fallacy is one which educated and uneducated alike may permit themselves to be deceived by; and that is, receiving gossip for fact, or relying upon their own judgment as to individual cases of reputed smallpox. Now it so happens that even medical men are often mistaken at the outset of a case of eruptive disease as to its real nature, and a variety of maladies of this class are taken to be smallpox which are not smallpox at all. Mr. Marson* gives a list of 185 cases of febrile diseases—not smallpox—which, in the course of sixteen years, were sent as cases of smallpox into the smallpox hospital: and cases of all kinds are usually seen by some medical men before they are forwarded there. The diseases which have been most frequently thus mistaken for smallpox are measles, febrile lichen, chicken-pox, and fever. A becoming modesty, then, would induce non-professional people to hesitate in deciding even upon the evidence of their own eyesight in the face of such a fact as this.

* 'Med. Chir. Trans.,' vol. xxxvi, p. 368.

Still it is a truth which is not to be questioned that successful vaccination is, in a proportion of instances, followed, after a lapse of time, by an attack of smallpox. A person may have the true vaccine disease and yet suffer from smallpox at a subsequent period of his life. Indeed, Dr. Jenner, in his very first work, mentions a case of the kind.* “Elizabeth Sarssnet lived as a dairymaid at New Park Farm in this parish. All the cows and the servants employed in milking had cow-pox, but this woman, though she had several sores upon her fingers, felt no tumours in the axillæ nor any general indisposition. On being afterwards casually exposed to variolous infection, she had the smallpox in a mild way.” Very early in the history of vaccination fears were expressed that the protection afforded by cow-pox would not be permanent. Hufeland, in the year 1800, was one of the first to give expression to such an apprehension, but his opinion was formed upon analogical considerations; and Woodvillet† alludes to this opinion as already making way in the minds of many persons. And it was not long before rumours got abroad confirmatory of such a view.‡ In 1800 and 1801 two cases of what we should call now modified smallpox came under the notice of Dr. Willan, six months after vaccination, and another case in 1804, the child having been vaccinated in 1802. The following year put the new protection to the test, for in that year a severe epidemic of smallpox fell upon the Metropolis, and then the doubt which hung over the subject was in a fair way of being settled.

The result of the epidemic was, on the whole, to confirm the belief in the protective power of vaccination; for the very

* Op. cit., p. 71.

† Observations on the cow-pox, 1800, p. 28.

‡ A number of cases of smallpox, either casual or from inoculation, in persons vaccinated for periods from six months onwards will be found collected by Dr. Thomson in his work on smallpox, pp. 157 *et seq.*

large majority of those who had been vaccinated—none of course being longer than six or seven years—escaped attack. Yet there were a few who were not exempted by the vaccine disease they had undergone ; but, as respects these, the observation was made that the smallpox which seized upon them ran a very mild course, and was in some instances of a character so benign as to mask very much the nature of the malady. In 1812 and 1813 the Central Committee of the French Academy and the National Vaccine Establishment in this country, while admitting the occasional occurrence of post-vaccinal smallpox, both expressed their belief on the rarity of the event. It is to be observed that Jenner and the early vaccinators performed the operation by means of a single puncture, following thus the practice which they were accustomed to in performing variolous inoculations. But at this time the National Vaccine Establishment recommended that multiple punctures should be made. In subsequent epidemic outbreaks observations were made by various persons as to the vaccination of persons attacked, as by Dr. Adams in the Forfar epidemic of 1813, by Harrison in the epidemic of 1817 at Kendal, and by various other observers upon the Continent ; and all testified more or less strongly to the fact of post-vaccinal smallpox, but established at the same time the comparatively mild and modified character which the disease under these circumstances assumed. The epidemic of 1817 and 1818 established the truth of the occurrence beyond all future question ; it has been from that time forward accepted among all professional men that smallpox may occur in a vaccinated individual, but that the disease is mitigated in severity.

But while admitting the truth of this, we by no means agree to the *use which has been made of hospital statistics* in regard to the frequency of the occurrence of post-vaccinal smallpox by some of the detractors of vaccination, by whom

the proportion of vaccinated and unvaccinated persons admitted with smallpox is pointed to as evidence that vaccination affords no protection against attacks of the disease. Thus, among 5748 patients admitted into the Smallpox Hospital between the years 1836 and 1851, there were 3094 who had marks of vaccination upon their arms, while the smaller number, 2654, had not been vaccinated. Out of 872 cases of smallpox which entered the Imperial Hospital at Prague between 1847 and 1856,* there were 819 who were known to be vaccinated persons, and only 43 who were unvaccinated. Out of 410 who entered the Hospital of the Brothers of Mercy in the same city, 370 were vaccinated individuals and 40 unvaccinated. I might go on multiplying examples, but it is needless, for it is no fair argument against the protective power of vaccination. Suppose that out of 100 persons run over in a year in the streets of London, it should be discovered, on inquiry, that only ten of them were deaf people, while ninety had the use of all their senses, would any man argue from this that the fact of the ninety being able to hear and see had anything to do with the accident that happened to them? It might turn out upon investigation that these ninety formed but a very small fraction indeed of the thousands, equally fortunate, who traversed London in a day, when compared with the proportion which the ten deaf persons formed to the number of deaf persons similarly exposed to danger. And so it is with vaccinated individuals. Before drawing any such conclusion as that I have referred to, the number of vaccinated and unvaccinated persons exposed to the peril of smallpox must be known. Suppose we were to tell these objectors of masses of persons attacked with smallpox, among whom the proportion of vaccinated and unvaccinated individuals was reversed? Such instances could be cited. I will quote one

* Papers, &c., p. 159.

or two. Out of 363 cases admitted into the Emperor Francis Joseph Hospital at Prague between 1854 and 1856, only seventy-five were vaccinated, and 188 unvaccinated. Out of 608 cases observed by Messrs. Bowerbank and Turner in Jamaica, only 120 were vaccinated, and 477 unvaccinated; and lastly, out of 6020 who are reported by Bousquet* to have had smallpox in the Marseilles epidemic of 1828, 2000 only had been vaccinated, while 4000 were unvaccinated. What would be said to us if we argued that such occurrences proved the protective power of vaccination? Of course they do nothing of the kind. The actual proportion of vaccinated and unvaccinated persons who suffered from smallpox in Prague during the period of twenty-one years alluded to has been estimated. I have mentioned it before. The proportion of attacks among the vaccinated class in that city was one in 367 individuals, while among the unvaccinated it was one in twelve and a half individuals. Again, in the epidemic of Marseilles of 1828, to which Hamernjk refers in support of his views, the vaccinated persons who suffered formed only one fifteenth of all the vaccinated persons in the city, while one half of the unvaccinated suffered from an attack. Indeed, if the argument of objectors drawn from the gross numbers of vaccinated and unvaccinated attacked with smallpox in the present day be worth anything, it should be pushed to its extremity, and the inference drawn that, not only does vaccination afford no protection, but that it promotes the subsequent occurrence of an attack of smallpox. Hamernjk, as I understand him, does go so far even as this. Logically, he is bound not to stop short of it.

But, after all, the occasional occurrence of smallpox after vaccination is not a thing to be surprised at, when we take into consideration the *analogies of the vaccine disease*. It is a thing which was even anticipated, as we have seen, upon

* Bousquet, op. cit., p. 195.

this very ground, by some of Jenner's contemporaries. Vaccinia is but one member of a group of exanthematous diseases in which non-recurrence is the rule, and a second attack in a lifetime the exception; and one of these is smallpox. With smallpox, vaccine as one of the varioloid maladies has the very closest analogical relations, such as in an earlier part of this essay I have striven to exhibit. So close is the relationship that the vaccine disease, when undergone, destroys that in the human system which imparts to it the capability of developing smallpox, just as an attack of smallpox was shown both by Jenner and Woodville, in the very earliest inquiries instituted, to destroy in man the capability of developing vaccinia. Now, although it is the rule that smallpox only occurs once in a lifetime, instances of recurrence are nevertheless occasionally met with. As a rule one attack of smallpox destroys for a lifetime the receptivity of the system for the same disease; but it does not always destroy it for a lifetime. Never does an epidemic of smallpox occur but instances are met with of persons suffering who have had the disease before. Thus it was in the Marseilles epidemic of 1828, in which it is calculated that one in every hundred of those who had suffered previously from the disease underwent a fresh attack.* So, too, out of 3681 persons admitted during sixteen years into the Smallpox Hospital, there were 47 who had had smallpox before.† Nor is it always the mildest attacks of smallpox which leave behind them a further capability of receiving the disease. Mr. Baker, in his reply to Mr. Simon's inquiries,‡ says that the worst case of smallpox he ever saw occurred in a person who was pitted by a previous attack. And instances are on record of repeated attacks, even of severe

* Bousquet, *op. cit.*, p. 195.

† Marson, *loc. cit.*, p. 366.

‡ Papers, &c., p. 38.

smallpox. One such case, related by Dr. Innhauser,* is sufficient to quote as an example, in which a woman who, when twelve years old, had an attack of smallpox which left behind it large, deep, and confluent scars, was seized a second time when she was forty-three years of age with an attack which again was sufficiently severe to leave scars distinguishable from those left by the first attack. Two years later she was attacked with smallpox a third time, but the attack was milder than the two former.† If, then, an attack of smallpox—a disease which so to speak is natural to man—does not always, even when severe, destroy for ever the receptivity of an individual who suffers it, it is surely no matter for surprise that, in occasional instances, vaccinia, which is not a disease natural to man, which is foreign to him, should fail to effect that which an attack of smallpox itself does not always accomplish.

But if vaccination does not invariably confer immunity from an attack of smallpox, *it exercises a most obvious and remarkable influence over the progress and issue of the disorder.* I have already mentioned that the mildness of the course of post-vaccinal smallpox was noted by some of Jenner's contemporaries. The terms now commonly in use to designate the disease, thus altered in its character, are "varioid" and "modified smallpox." The fever which ushers in the disease may, indeed, be urgent, and the eruption appear at the usual period; it may even be abundant in quantity, and, although commonly discrete, it may be confluent. So far

* 'Brit. and For. Med. Chir. Rev.,' vol. xxiv, p. 541.

† An elaborate collection of instances of secondary smallpox made by Dr. Andrew Smith will be found in the Appendix to Dr. John Thomson's work on smallpox (1822). Dr. Jenner ('Further Observations,' &c., p. 44) says,—“The constitution cannot by previous infection be rendered totally unsuceptible of the variolous poison; neither the inoculated nor the casual smallpox, whether it produces the disease in a mild or in a violent way, can perfectly extinguish the susceptibility.”

the mitigated resembles the unmitigated disease. The difference is observed in its subsequent progress. The progress of the pustules after the second or third day of the eruption is irregular; in some parts they may ripen rapidly, while in other places they cease to enlarge, but begin to shrivel and dry up; or no matter at all may be found in any of the pocks, but in place of this they may harden into small tubercular elevations which after a time disappear. Where pus is found in the pocks and pits are left, the latter are very much smaller than those produced by regular smallpox. Another characteristic of the mitigated disease is absence of consecutive or secondary fever. This form of disease is not commonly fatal. Now, let me be understood. I am not saying that this mitigated form of smallpox is a bastard product of the virus of smallpox, which is so peculiar to vaccinated persons that it only arises when the virus operates upon constitutions which have previously been affected by the vaccine disease. Hamernjk and other detractors of vaccination are quite correct when they assert that smallpox presenting similar mitigated characters was more or less familiar to physicians prior to Jenner's discovery.* At all periods, smallpox, like other exanthematous diseases, has presented itself with various degrees of severity in different individuals, depending upon their inherent capability for developing the malady; and in proof of the modified variety of smallpox not being the result of vaccination alone, M. Bousquet† quotes a statement of M. Pariset to the effect

* In the same family, where smallpox enters it, cases may be met with of a modified and unmodified character. In some cases an attack of varioloid may be distinctly traced to some definite exposure to the contagion of smallpox and inoculation of smallpox virus has been followed in cases on record by the varioloid form of the malady. Cullerier also found that children who had suffered from varioloid were insusceptible of smallpox inoculation (Bousquet, *op. cit.*, p. 209).

† *Op. cit.*, p. 186.

that he met with it in Egypt, in the islands of the lake Menzelech, where vaccination has never penetrated. All that is contended for is, that the previous occurrence of the vaccine disease is one of those circumstances which so far destroys the capability of developing the smallpox virus as to favour its assumption of this mitigated form when the receptivity is such as to permit of the virus operating at all.* The frequency with which the attack of smallpox was modified in persons admitted, after vaccination, into the Smallpox Hospital is shown in Table 8 (see Appendix). Out of 3094 persons thus admitted, the disease is stated to have been "modified" in its course in as many as 2149, that is to say, $69\frac{1}{2}$ per cent. of the entire number: the modified disease was confluent in 529, semiconfluent in 436, and distinct and varicelloid in 1184. On the other hand, out of 2654 unprotected cases, a modified form of the disease was only noted in 69, that is to say, $2\frac{1}{2}$ per cent. of the entire number. Again, out of the 945 vaccinated persons who had the unmodified form of the disease, the eruption was confluent or semiconfluent in 837, or in 88 per cent.; while, out of the 2585 cases of unprotected persons who underwent the unmodified disease, the eruption was confluent or semiconfluent in 2419, or in $93\frac{1}{2}$ per cent. Thus it appears that, although unmodified smallpox even does appear in about a third of the vaccinated persons attacked, the influence of the previous vaccination is not absolutely lost, for that the disease is less commonly confluent than among those who have not been vaccinated.†

* This was exhibited in a striking manner in the Provence epidemic of 1828, when smallpox was introduced into the seminary at Digne by one of the pupils, and attacked thirty of them; but with this difference, that the disease took the ordinary character in those who were not vaccinated, but of the "varioid" in those who were.—Bousquet, op. cit., p. 201.

† Among the cases of post-vaccinal smallpox which occurred in the Norwich epidemic, Cross mentions one in which it assumed the petechial character; but

Again, the comparative mildness of the attack of smallpox which vaccinated persons undergo when the disease chances to seize them, is further shown by the greatly *reduced fatality of the malady*. The evidence that can be adduced in proof of this truth is so abundant as to be absolutely embarrassing. The records of every epidemic and of every hospital I have seen testify to it; and, although in some of these records the result is exaggerated to a certain extent by counting in cases of chicken-pox among those of smallpox, this only vitiates in a very trifling degree the general result, and not at all the final inference. It may be sufficient that I should refer again to the abstract of Mr. Marson's table in the Appendix (Table 10), and to one which has been drawn up very carefully from various reliable sources by Mr. Simon (Table 9). From the former it appears that, during sixteen years, the fatality of smallpox in the hospital among the unprotected was 35.55 per cent., while among the vaccinated it was only 5.25 per cent. From the latter it appears that, while among the unprotected the fatality of smallpox varied in different places from 60 to 14½ per cent., among the vaccinated it never reached 12 per cent. of those attacked, and in some instances the post-vaccinal smallpox was not fatal at all.

But as epidemic seasons, after all, supply the best test of the protective power of vaccination, let me refer to the results obtained in some of those on record. I will only take a few just as they come to hand.

he adds that such cases "can have no weight against the practice of vaccination, when compared with 10,000 vaccinated individuals living in the midst of a contaminated atmosphere, with 530 deaths among little more than 3000 who had neglected vaccination, and with the occasional occurrence of regular smallpox in those who formerly had the disease, either naturally or by inoculation."—Op. cit., p. 65.

Epidemics.	Total cases.	Deaths.	Vaccinated.			Unvaccinated.			Previous smallpox.		
			Cases.	Deaths.	Death-rate per 100.	Cases.	Deaths.	Death-rate per 100.	Cases.	Deaths.	Death-rate per 100.
Norwich, 1819*	202	46	2	0	0	200	46	23.0			
Marseilles, 1828†	6020	1024	2000	20	1.0	4000	1000	25.0	20	4	
Rouen, 1864‡	821	74	620	8	1.2	201	66	32.8			
London Smallpox Hospital admissions, Epidemic of 1866§	2037	270	1605	118	7.3	425	152	35.7			
Prague, 1840-41	5209	465	2721	83	3.0	2488	382	15.3			
Jamaica, 1851¶	608	81	120	4	3.3	477	75	15.7	11	2	
Edinburgh, 1863** (Royal Infirmary)	330	45	206	4††	1.9	124	41	33.0			

* Cross, op. cit.

† Bousquet, op. cit., p. 195.

‡ 'Report of French Academy on Vaccination,' for 1864, p. 131. (M. Bouteilles.)

§ 'Report of Smallpox Hospital for 1866.'

|| Papers, &c., p. 162.

** 'Med. Times and Gazette,' 1863, vol. ii, p. 45.

†† Three of these deaths were not fairly attributable to the smallpox, one died from delirium tremens, one from puerperal mania, and a third, an old woman of 50, from bronchitis.

¶ Ibid., p. 140.

The difference of result is here again very striking. But this table shows us something more. It exhibits to us what the fatality of the several epidemics would have been had it not been for the vaccination of the people, and what it became in consequence of the vaccinations previously performed. Smallpox among the unprotected has lost nothing of its original virulence; but, taking the masses of people among whom it now breaks out, vaccinated and unvaccinated together, it has become a less fatal disease than it used to be among those attacked by it. Thus we learn from Dr. Gregory,* that during the twenty-five years preceding the introduction of vaccination, the fatality of smallpox in the hospital at Battle Bridge was 32 per cent. of all that were attacked.† In the epidemic of 1838, out of 694 persons admitted, 188 died, or 27 per cent. Of these, four sevenths only had been vaccinated previously. This is one of the London epidemics that Hamernjk alludes to as showing the inefficacy of vaccination. In 1842-4 the fatality of smallpox was 21 per cent., in 1851 it was 15 per cent., in the severe epidemic of 1863 it was 17 per cent., and in 1866 it was 13 per cent. Thus, as vaccination has progressed among the people, the fatality of the disease, as shown by the records of this hospital, has gradually lessened. Foreign observations confirm the fact of the generally less fatal character of smallpox. Assuming one in three or one in four as the ratio of fatality prior to the introduction of vaccination—and it cannot be said to be an exaggerated estimate of it—we have from continental sources the following results :

* 'Library of Medicine,' vol. i, p. 305.

† The site was insalubrious, and the building not in accordance with modern sanitary views, but the extra fatality from these causes may have been compensated by mild inoculation cases admitted.

108 FATALITY OF POST-VACCINAL SMALLPOX.

In the epidemic of Carouge,* 1828, the fatality was 10·7 per 100 of attacks.

”	”	Geneva,*	1832	”	”	10·5	”	”
”	”	”	* 1845	”	”	5·0	”	”
”	”	”	* 1858-9	”	”	10·8	”	”
”	”	Vevey,*	1858-9	”	”	5·5	”	”
”	”	Rouen,†	1864	”	”	11·0	”	”
		Throughout France in	1861‡	”	”	18·0	”	”
		”	1862	”	”	13·5	”	”
		”	1863	”	”	10·9	”	”
		”	1864	”	”	11·1	”	”
		Wirttemberg, 1831, 1836		”	”	11·8	”	”

We may now go back again to Mr. Marson’s tables—a complete storehouse of information upon this subject—and see how the vaccinated fared as compared with the unvaccinated, when they suffered from “unmodified” smallpox :

Out of 2585 cases of smallpox, “unmodified ” in unprotected persons, 985 died, or 38·1 per cent.

Out of 945 cases of smallpox, “unmodified ” in vaccinated persons, 231 died, or 24·4 per cent.

Even here, then, when the disease pursued its regular course, the fatality among the vaccinated was one third less than it was in the case of the unprotected.

I think, after this amount of evidence, sufficient has been said to prove the comparative mildness of post-vaccinal smallpox.

The effect of a previous vaccination in the presence of an attack of smallpox is thus put by Steinbrenner :

“Semblable à un arbre dont on a coupé les racines au printemps, après que la première sève fut montée dans les bourgeons : ceux-ci d’abord se développent, le feuillage

* ‘ Marc d’Espine, Arch. Gén.,’ 5 ser., t. 14, p. 74.

† Bouteilles ‘ Rept. of Acad. Imp. de Méd.’ for 1864, p. 131.

‡ Ibid. for the several years referred to.

|| Steinbrenner, ‘ Traité sur la Vaccine,’ p. 833.

commence à s'étaler sous l'influence des conditions favorables que la saison établit, mais tout-a-coup la sève venant à manquer, le développement des feuilles s'arrête, elles se fanent, se dessèchent et péricassent en peu d'heures, faute de nourriture. De même, le virus variolique trouvant un terrain qui lui offre une plus ou moins grande quantité de nourriture, de sève, il s'en empare avec avidité, et développe plus ou moins bien, suivant la quantité d'aliments qu'il trouve, une variole plus ou moins intense, mais au beau milieu de son développement, un peu plus tôt, un peu plus tard, le travail morbide cesse, les symptômes généraux au lieu de gagner en intensité, disparaissent comme par enchantement, et l'affection locale, qui, dans la variole des non vaccinés, parcourt d'une manière si régulière toutes ses périodes, reste stationnaire, les pustules se fanent, se flétrissent, tantôt à l'état de simples papules, tantôt avant d'avoir atteint la période de suppuration ; tantôt enfin, quand un commencement de suppuration a eu lieu, elle ne s'achève pas, et la formation des croûtes avorte." (p. 463.)

It is scarcely worth while to contend with those who, dissatisfied with the protective power conferred by vaccination, would counsel *a return to the practice of variolous inoculation*. The late Dr. Gregory was, I believe, almost the last person of any eminence in this country who considered that it was desirable under any circumstances. But as I have already casually noticed the subject, when discussing the national mortality from smallpox at various periods, and shown how greatly it increased during the half century when inoculation was practised,* it may not be amiss to complete

* It is, however, only fair to the memory of Dr. Gregory, and to those who do not think that inoculation is worthy of entire reprobation, to say that they object to this argument on the ground that inoculation never was the general operation that vaccination has become, and hence the unprotected many suffered from the contagion generated in the protection of the few. Dr. Copland argues that, "if inoculation were generally adopted at a proper age,

what I have further to say about it here. That smallpox inoculation did confer a remarkable immunity from further attacks of the disease is unquestionable, and, prior to the time of Jenner, it did inestimable service to those who underwent the inoculated disease in a favorable manner, as most persons did. Individually then it was advantageous; nationally, statistics have demonstrated that it was not an unmixed good. But what I am now concerned about is the comparative protection it conferred when looked at side by side with vaccination. Apart from the fact that a proportion of those inoculated die—a proportion differently stated, from 1 in 100 to 3 in 1000, it is certain that, as with vaccinated persons, it fails sometimes to afford protection. How often it is not easy to say; but as respects the protective power of smallpox inoculated or contracted naturally, there are some statistics which I may quote.

In Dr. Seaton's pamphlet he mentions 82 medical men who were inoculated in infancy; and although constantly in the habit of attending cases of smallpox, only 3 or 3·6 per cent. took the disease.

Dr. Balfour relates that, during forty-eight years, there were 1950 boys admitted into the Royal Military Asylum, having upon them marks of previous smallpox. Of these 12 had smallpox again while resident, these being in the ratio of 6·15 per 1000.

At the time of the Marseilles epidemic, 1828, there were estimated to be living among the population 2000 persons, up to thirty years of age, who had previously had smallpox. Of these 20, or 1 in 100, were attacked during that outbreak.*

there could not possibly be the pabulum for an epidemic outbreak, and scarcely the occurrence of a natural case."—'Dict. of Med.' (abridged), p. 1196. But half a century failed to render the practice *general*.

* M. Honorat calculated that in Digne, during the same epidemic season.

Just as after vaccination, then, it would appear that the chances of a second attack vary with the degree and chances of exposure to contagion; but, on the whole, the chances of avoiding a second attack are in favour of those who have undergone smallpox, either by inoculation or naturally, rather than of those who have undergone the vaccine disease. Among the boys at the Military Asylum, the proportion of cases of smallpox after vaccination was 7·06 in the 1000. Among the medical men polled by Dr. Seaton, the proportion of cases of smallpox after vaccination was 12·6 per cent., and in the Marseilles epidemic the proportion was 6·6 per cent.—more than six times greater than the frequency of secondary smallpox. But when we come to estimate the severity of the attacks of smallpox in its secondary attacks, and of smallpox after vaccination, a different complexion is put upon the affair.

During 16 years, 27 persons who had had inoculated smallpox were admitted into the Smallpox Hospital. Of these 12 had the disease in an unmodified form, that is, 44 per cent.; whereas the percentage of “unmodified” cases after vaccination was 30½. Five out of 14, or 35·7 per cent., had the disease after natural smallpox in an “unmodified” form. Then again as to the fatality of the disease. Of the 27 who had smallpox after inoculation, 7 died, or 23 per cent., whereas the fatality of post-vaccinal smallpox was 5·25 per cent. Still it afforded some protection against the severity of the disease, inasmuch as 35·5 per cent. of those who were neither vaccinated nor variolated succumbed to their attack. Of the 14 who had a second attack of natural smallpox, none are stated on Mr. Marson’s table to have died. Again, of the 12 boys who had secondary smallpox at the Royal Military Asylum, 4, or 33 per cent., died; 12 cases of smallpox occurred amongst 2000 who had previously had the disease.

while of the 27 who had post-vaccinal smallpox, none died. And lastly, of the 20 secondary cases in the Marseilles epidemic, 4, or 20 per cent., died, while of those who had post-vaccinal smallpox, 20, or 1 per cent. only, died. I shall now quit this subject.

Let me repeat—for in a discussion such as this it is a thing we are very apt to lose sight of—that I am now considering the case of the minority, and not of the majority of those who have undergone a successful vaccination. With the majority, placed under ordinary circumstances, not unusually exposed to catch the disease, vaccination is a lifelong protection against smallpox. In *epidemic seasons* vaccinated persons are, however, more liable to take it than they are at ordinary times. But this is not because they are under the special influence in these seasons of some peculiar condition of the atmosphere. If it were so, they would be attacked as early in the epidemic as those who are not protected. But this is not the case. It is the unprotected who suffer first, and the vaccinated are not invaded until the foci of contagion have thus been multiplied, and the intensity of the contagious force brought to bear upon them has been heightened.* There are some interesting observations by Dr. Buchanan† upon this matter. In the district of St. Giles and Bloomsbury, where that gentleman holds the post of health officer, a register of smallpox was kept during the epidemic season of 1863. “The disease began to prevail in January, 1863, and reached its highest point in March. In January the unvaccinated were three quarters of the whole number of cases; in February they were more than half; in March they were little more than a third of the whole; while in April they formed scarcely more than a quarter of the cases coming

* It is also during epidemic seasons that secondary smallpox most frequently occurs.

† ‘Sixth Report of the Medical Officer of the Privy Council,’ p. 109.

under treatment. In St. Pancras, in so far as the vaccination of the smallpox cases are recorded, three quarters were unvaccinated among those admitted into the workhouse wards the first month of their opening, while the general proportion of the unvaccinated was only one in every six cases admitted." He says also, "Where we visited a union that had suffered some months from the epidemic, we were assured, as constantly, that the largest number of attacks had been among the vaccinated; but it was almost invariably added that, at the commencement of the epidemic, the unvaccinated were chiefly attacked."

Having now, as it will probably be considered, sufficiently established the fact that the majority of vaccinated persons are to all practical purposes absolutely protected against smallpox, and that, when smallpox does occur subsequently to the operation in the minority, it assumes for the most part a mild or modified character, let us look a little more closely into the matter, and inquire how soon after vaccination a person may be regarded as safe from attack, and whether there is any evidence to show that the protection he receives wears out in any degree with lapse of time. Important practical questions these, which demand at our hands the fullest consideration.

But in order to consider them at an advantage, a decision must be come to upon a preliminary question, namely—

Upon what does the perfection of protective power in the vaccine disease depend?

In my introductory remarks I have taken some pains to show that the perfect development of vaccinia, as well as the

regular development of smallpox, is marked by a double series of phenomena, both maladies being members of the same group of exanthematous diseases. I have shown that when the virus of smallpox is inoculated the disease in the punctured spot pursues, apparently, a local course up to about the eighth day from inoculation, and that then symptoms are manifested of a general constitutional disturbance, which terminate in an eruption of smallpox more or less generally over the whole body. So, too, I have shown that when the vaccine virus is inoculated the disease in this instance also pursues, apparently, a local course up to about the eighth day from vaccination, and that then evidence, more or less distinct, of general constitutional disturbance is manifested, which consists in febrile excitement, slight or active, the formation of an areola around the vesicle, and perhaps some lymphatic glandular swelling. I have also dwelt upon the fact that both smallpox and vaccinia, although in a different degree, destroy the capability of developing smallpox in the system when the virus gains access to it, either by inoculation or casual exposure to the influence of the virus. I have suggested that vaccinia does so by virtue of its operation upon the same elements of the blood and in the same manner as an attack of smallpox. I may briefly sum up the view which I adopt of the preventive relation in which the vaccine disease stands to smallpox in the words of Bousquet:—“When we look at their effects, it cannot be said that vaccine cures smallpox, nor, strictly speaking, can it be said that it *prevents* it; it stands in lieu of it—there is a *substitution*, and nothing more.”*

When, therefore, the question is asked, Upon what does the protective power of the vaccine disease depend? I answer, *Upon the perfection of its development, on the whole, as an*

* ‘Traité de Vaccine,’ p. 119.

exanthematous disease; upon the closeness of the similarity it exhibits to the development of smallpox; upon the perfection of the *substitution*. I have no sympathy whatever with those who take one-sided views upon this subject, who exalt the importance of the constitutional disturbance, and regard slightly the perfection of the local phenomenon; nor yet with those who, disregarding the former, consider the eruption to be all in all. I look upon both as important, because the two are pathologically associated, and I think I shall be able to show that experience bears me out in the opinion which I have formed.

In order to render the observations I am about to make as clear as possible, I shall consider separately, however, 1st, the evidence which exists of the importance for protective purposes of a full and complete development of the local phenomena; and then, 2ndly, the arguments which may be put forward in favour of the protective influence exerted by the constitutional disturbance.

1. *As to the local phenomena*—the fulness of the eruption, the number and character of the vaccine vesicles.

The first question that arises is the degree in which protection from smallpox is dependent upon the *character* of the vesicles produced by vaccination. That these vary considerably in size and in progress in different individuals is well known to every vaccinator; but as vaccination is usually performed in infancy, the grown-up person can have no recollection of the sort of eruption which was produced upon his arm, and the reports respecting it which he may have heard from his parents are about as worthy of reliance as any other parental opinion of the perfections of their children. Happily, however, the vaccine disease leaves its own history behind it in the scars, which remain permanently upon the arms. When the vesicles have been good and normal the scars left behind will be good, and when the

vesicles have been unsatisfactory the scars will be small and imperfect. It is thus that the cicatrices upon the arm become employed by medical men as the indication of the former character of the local eruption. There are several modes of judging of the goodness of a scar. One of these is the distance from the individual at which it may be seen. A good scar may be seen almost across a room, while the worst scars require close inspection of the arm to distinguish them at all. Then the size may be measured, a plan adopted by Dr. Sanderson* in his examination of school children; but in using this index it is necessary to be aware that very large single scars are produced by some vaccinators by their mode of performing the operation, the single scar resulting, not from one single vesicle, but from a group of confluent vesicles. Another index is the depth and appearance of the surface of the scar—whether it is superficial or sunk beneath the level of the surrounding skin, smooth or dotted with smaller depressions (foveæ). According to Marson, “a good vaccine cicatrix may be described as distinct, foveated, dotted, or indented, in some instances radiated, and having a well- or tolerably well-defined edge; an indifferent cicatrix as indistinct, smooth, without indentation, and with an irregular or ill-defined edge.”

There are three tests which are applicable in judging respecting the amount of protection any person has received. They are—1, the subsequent inoculation of smallpox virus; 2, the result of casual exposure to contagion; and 3, an attempt to reproduce the vaccine disease by re-vaccination. Let us stop a moment to inquire into the value of these several tests. As to the value of the inoculation test there is no doubt at all; it was largely used by the early vaccinators, and has been occasionally used since. It indi-

* ‘Sixth Report of Medical Officer of Privy Council,’ p. 205.

cates unmistakably whether the individual is or is not capable of developing smallpox on the virus being introduced into the system; but as an ordinary test of protection, its use is, of course, unwarrantable; and, besides, variolous inoculation is by statute an illegal proceeding.

Next, then, we come to the test of *actual taking of small-pox on exposure* to contagion in the ordinary way. We may ask, for instance, whether persons with good scars on their arm are less frequently attacked with smallpox than people with bad scars on their arm. But then there is this fallacy to which any statistics on this subject are liable, namely, that the persons with good or bad scars respectively may not have been equally exposed to variolous infection; it is to be presumed, in arguing from them, that they have been equally exposed, and where large numbers of persons are in question probably this presumption may be justified. It is altogether another matter to accept, as a *precise* index of the liability to suffer, the proportion of persons with good or bad scars who are attacked with smallpox—for example, the number of persons received with good and bad scars respectively into a hospital. In arguing from such numbers, we must be careful not to fall into the error which we condemn in those who argue against the value of vaccination from the number of vaccinated persons compared with unvaccinated who catch the disease.

Lastly, there is the *revaccination test*; and the value of this test demands full consideration, because as an experimental test it is the only one left us, and we must be careful not to deduce from its results more than experience warrants. It depends upon this—that inasmuch as the vaccine disease destroys the capability of an individual for the development of the smallpox virus, the return of such capability may be judged of by the possibility of producing again the vaccine disease—the substitute of smallpox, that

which acts upon the same elements in the system as smallpox virus acts upon when it produces smallpox on inoculation. If smallpox virus is capable upon inoculation of producing smallpox, vaccine virus on inoculation ought to be able to produce vaccinia. This is the basis of the test. I say emphatically, "capable *on inoculation* of producing smallpox," for this is the utmost extent of the value of the test. Revaccination, as a test of protection, is a test of protection against smallpox when we know that the virus has with certainty been introduced into the system. Nothing more. Its success does not show that an individual on exposure to casual contagion is equally likely with a wholly unprotected person to take smallpox. For, as I have before pointed out, there is in casual exposure a second element to be considered, namely, the capability, not of developing the virus when introduced, but the capability of receiving it into the system by ordinary means at all. And again, I have shown that the quantity of the morbid virus—and hence, probably, also the duration and intensity of the exposure to contagion—is a point not to be overlooked. A person who has undergone a primary vaccination alone is unquestionably protected against all ordinary liabilities, such as unvaccinated persons would suffer under; and it requires unusual exposure in the former to cause the development of smallpox. I have shown this in the case of the vaccinated and unvaccinated at the outbreak of an epidemic; and it is seen in the fact that, in the very height of an epidemic, the very large majority of the vaccinated remain free from seizure. In revaccination we know we introduce a virus capable of operating upon that within the system which renders it capable of redeveloping vaccinia or smallpox; on casual exposure of a vaccinated person to smallpox contagion we are not sure that the contagious principle finds its way into the system at all, and if it does not, why, the disease cannot be developed; as soon might we

expect a grain of corn to germinate when deposited upon the bare surface of a granite rock. If the chances of contracting smallpox after vaccination and those of producing vaccine disease a second time by revaccination were equal, we should expect to find that, in epidemic diseases, the occurrence of smallpox casually after vaccination would be as common an event as success in revaccination. But what is the fact? In Heim's revaccinations in the Wirtemberg army a success, good, bad, or indifferent (see Table 12) was attained in 57 per cent., whereas of all the vaccinated persons exposed to contagion during the Marseilles epidemic of 1828 only 1 in 367 was attacked with smallpox. Even when persons have been protected against smallpox by a previous attack, vaccination succeeds in a large percentage of cases. Thus, during the revaccinations in the kingdom of Wirtemberg from 1831 to 1836 there were 297 persons who had had smallpox that were vaccinated, and a complete success was attained in 32 per cent., and a modified success in 26 per cent.; together, a good or imperfect success was attained in 58 per cent.* But this subject is so important, and the popular opinion upon the matter so strong, that I cannot avoid instancing some remarkable examples of vaccination, succeeding not only after smallpox, but after frequent exposure to smallpox contagion. Thus, Woodville has seen vaccinia contracted by contagion from the cow in an adult who had had smallpox in infancy, and even succeeded in vaccinating a woman who had undergone the disease. It is stated by Steinbrenner that both Professor Breschet and Professor Moreau, who both had smallpox in infancy, and had escaped a second attack from the innumerable exposures they had undergone in the course of their professional life, were nevertheless successfully vaccinated afterwards; and Heim, of Wirtemberg, although

* Theil , of Kasan, in vaccinating variolated subjects, obtained a complete success in 19 per cent., and a modified success in 6 per cent.

he had smallpox in his youth, and exposed himself with impunity to contagion, at last even nursing his own brother through an attack of smallpox, was nevertheless only three weeks after the last event vaccinated with success.

With this estimate of the value to be attached to these several tests, I may proceed to consider the relation which the *character of the vaccine vesicle, as evidenced by that of the scar, has to the protection of the vaccinated person.*

Now, it is a matter of fact, which I suppose no one conversant with smallpox will be disposed to deny, that sometimes smallpox appears in persons whose arms exhibit one or more scars of the most perfect and typical character. On the other hand, there are persons who appear to be protected from the disease under casual exposure whose scars are more or less imperfect; indeed, if even imperfect scars did not ordinarily protect, what would be the spread of smallpox among the large masses of our towns in epidemic seasons, when we keep in mind the revelations made of the kind of scars to be found on the arms of most of the children in the parish and charitable schools in England? Why, in the schools of London, where certainly vaccination is not so badly performed as it is in some places, only 616 per 1000 of the children had good and satisfactory scars upon their arms (from one to four). Still, one may accept both these facts as truths, and yet maintain that the character of the vesicles produced by vaccination is an important matter when protection is in question. This is a point insisted on by Mr. Marson. Referring to Table 4, in the paper by that observer, from which I have quoted before, we find on adding up his numbers that there were admitted into the Smallpox Hospital during sixteen years 1765 persons with "good" scars upon their arms, and 1022 with "indifferent" scars upon their arms; that is to say, 633 per 1000 of the vaccinated admitted had good scars. Are we to conclude at once from

this that good scars do not protect better than indifferent ones; or, rather, that persons with good scars are more liable to take smallpox than persons with bad scars? Certainly not. For the proportion of persons with good and bad scars respectively among the population from which that of the hospital is recruited must be taken into account. Now, it is worthy of notice that this number, 633 per 1000, does not differ materially from that of the number of vaccinated children with good scars found by Drs. Seaton and Buchanan in the London schools, viz. 616 per 1000; the trifling difference that exists may be accounted for by the fact that all the admissions into the hospital are not of residents in London. So far as this fact goes, then, it tends to show that persons with good and bad scars respectively stand, so far as liability to receive and develop smallpox is concerned, independently of the number of their scars, much in the same position.* But when we come to inquire about the severity of the attack in each class, we learn in what respect those with good scars are better protected than those with bad scars. Thus, we find that in those persons who bore good scars upon their arms the frequency with which the smallpox was "modified" was greater than in those who had indifferent scars upon their arms.

Out of 1765 patients with *good* scars the disease was modified in 1383, or 78·4 per cent.; unmodified in 382, or 21·6. Out of 1022 patients with *indifferent* scars, the disease was modified in 655, or 64·1 per cent.; unmodified in 367, or 35·9 per cent.

Again, the fatality of the smallpox was greater in those persons whose vaccine scars were indifferent than in those in

* This result agrees with the opinion expressed by Steinbrenner that the proportion of those with good and bad scars attacked in epidemic seasons is about equal. I am not aware, however, that the fact has ever before been brought out numerically.

whom they were good. Omitting cases which were fatal from superadded disease, we find that—

Out of 1738 patients with *good* vaccine scars there died 80, or 4·6 per cent. Out of 1002 patients with *indifferent* vaccine scars there died 111, or 11·0 per cent.

The advantage of vesicles so perfect as to leave characteristic scars over vesicles so poor as to leave bad scars is seen, then, not in the absolute protection against an attack of smallpox which the one affords being superior to what the other affords, but in the fact that when the disease does happen its severity is not equal in the two instances, that it is milder in its course where the vaccine vesicles have been previously more perfect.

The application of the revaccination test gives a similar result. Table 11 shows the results of the revaccination of 1872 recruits in the Wirtemberg army in 1836, and I have calculated the per-centage of disease in those whose arms exhibited good and bad scars respectively. The result is that nearly an equal proportion of both classes were found to be capable of redeveloping the vaccine disease; what little advantage the one had over the other was on the side of those with good scars.* The conclusion to be drawn is probably this, that, other things being equal, persons with good scars on their arm and persons with bad scars, at the age similar to that at which these recruits entered, would not differ very greatly in the readiness with which they would develop smallpox on the virus finding its way into the system.

* In Table 12, of the Wirtemberg army vaccinations during a period of five years, the advantage of those with good scars over those with bad scars on their arms, as regards success of revaccination, is more apparent; 590·9 successes per 1000 attempts being obtained in the case of those with good scars, and only 539·7 in the case of those with indifferent scars. Still, even in this Table the difference is not very great, and is no more than may be accounted for without assuming that good vesicles are less protective than bad ones. It is a point to which I shall have to recur.

I may mention as an observation confirmatory of one of the inferences I have drawn, and which may be taken for what it is worth, that in India the practice of vaccination has been as successful in protecting from casual smallpox contagion, and from the operation of the virus introduced by inoculation as it has been in England; and yet it is stated that the vesicles as they appear on the arms of native children, and also the succeeding scabs, are a good deal smaller than those which appear upon the arms of European children also vaccinated in India.* Indeed, the revaccinations stated to have been practised by Assistant-Surgeon Russel at Amjere were much less successful than those which have been practised in Europe. Out of eighty-seven revaccinations practised from eight to forty years after primary vaccination, only five are noted to have succeeded.†

The next point relates to the *number of the vesicles*. And here, again, I have to remark that smallpox does not always spare in its attacks persons who have multiple scars of vaccine upon their arms; while, on the other hand, a single vesicle has in innumerable instances served as a protection both against the inoculated virus and against the influence of casual contagion. In fact, it was the protective operation of single vaccine vesicles upon which the reputation of vaccination, as performed by Jenner and the earlier vaccinators, was originally built up. But still we shall see that the number of vesicles produced by vaccination is not an unimportant element in the protection afforded. Referring again, then, to Mr. Marson's table, we find recorded upon it, as having been received into the Smallpox Hospital, 1357 patients with *one* vaccine cicatrix upon the arm, 888 with *two* cicatrices, 274 with *three* cicatrices, and 268 with *four* or more cicatrices; that is, out of the total number

* McLennan, in Dr. Duncan Stewart's work, before cited, p. 157.

† Dr. Duncan Stewart, *op. cit.*, p. 165.

of 2787 patients with cicatrices on their arms, 486·9 per 1000 had one, 318·6 per 1000 had two, 98·3 per 1000 had three, and 96·2 per 1000 had four or more vaccine scars. Now, let us see how this proportion agrees with that observed in the London schools by Drs. Seaton and Buchanan. They examined altogether, in different parts of London, the arms of 20,337 children, and they found that out of every 1000 vaccinated there were, on an average, 242·8 with with one scar, 398·3 with two scars, 226·5 with three scars, and 132·4 with four scars or more. To show the relation between these two sets of numbers we may place them in parallel columns.

	Proportion of scholars per 1000.	Proportion of patients per 1000.	Relation of proportion of patients with smallpox to proportion of scholars with 1, 2, 3, or 4 cicatrices.
With 1 vaccine cicatrix	242·8 . . .	486·9 . . .	Nearly double.
„ 2 vaccine cicatrices	398·3 . . .	318·6 . . .	$\frac{1}{3}$ th less.
„ 3, 4, or more do.	358·9 . . .	194·5 . . .	Nearly $\frac{1}{2}$ less.

Putting the quality of the vaccine vesicles, then, out of the question, it appears from this comparison that the production of two vesicles is more than twice as protective as one vesicle, and the production of three or more vesicles nearly four times as protective against the casual contagion of smallpox. But what when these several classes of persons do chance to contract the disease? Is there then any difference to be observed in its progress? In order to reply to this, I have made the following calculation from Mr. Marson's table, taking the death-rate from his calculation.

Admitted into Smallpox Hospital.	Number.	Unmodified smallpox.		Modified smallpox.		Rate per cent. of mortality after deducting cases affected by super- added disease.
With 1 cicatrix	1357	460	33·9	897	66·1	7·57
„ 2 cicatrices	888	213	23·9	675	76·1	4·13
„ 3 „	274	50	18·2	224	81·8	1·85
„ 4 or more	268	26	9·7	242	90·3	0·74

From which it appears that *unmodified* smallpox becomes more and more rare among vaccinated persons, according as the vesicles which are produced upon their arms are more and more numerous, while the modified disease becomes more and more frequent. The fatality of the disease follows the same order. Where there were *two* cicatrices upon the arm the death-rate per cent. was nearly half what it was when there was only *one*; when there were *three* it was one fourth, and when there were *four or more* cicatrices it was about one tenth of the death-rate of those with one cicatrix. Nothing can possibly be more convincing than this. These results correspond with what the analogy of secondary smallpox would have led us to anticipate. It is a general belief that secondary smallpox occurs more frequently in those in whom the primary attack was distinct than in those in whom the eruption had been more abundant and confluent; and so, more frequently after inoculated smallpox, when the eruption is usually moderate in amount than after natural smallpox, which is more apt to be confluent. Then, again, as regards the severity of the second attack, we can refer to Mr. Marson's tables. The numbers which were admitted into the Smallpox Hospital with secondary smallpox was indeed small, but the contrast between the progress and issue of the cases of smallpox after inoculated (discrete mostly) smallpox, and after smallpox naturally contracted (more severe), is so striking as to impart a value to it. Thus, out of 27 cases occurring after inoculated smallpox, 12, or 44 per cent., assumed the severe or unmodified, confluent or semi-confluent form; and 23.07 per cent. of the entire number died from the disease. On the other hand, out of 14 cases occurring after natural smallpox, only 5, or about 36 per cent., had the unmodified form of the disease, and all 14 patients recovered.

We may now inquire into the results of the application of the revaccination test.

In Table 11 (see Appendix) I have calculated the percentages of success or failure given in Heim's table, as respects those who presented from one to twelve vaccine cicatrices among the recruits of the Wirtemberg army re-vaccinated in 1836. Taking, as before, those with good and indifferent cicatrices together, and regarding the results as indicating the susceptibility of each class to develop smallpox on the virus gaining access into the system, the results are as follow. The complete failures to redevelop the vaccine disease were least of all in those whose arms exhibited but one vaccine cicatrix; they were most frequent of all in those with eight cicatrices and upwards. It is observable on the table that the failures were less and less frequent as the scars became more frequent from two to five. This does not necessarily prove that a larger number of vesicles than two does not afford a greater protection, inasmuch as it is not unlikely that, in all these instances, four or five punctures were originally made, and then the number of vesicles that arose might have been in part determined by the susceptibility of the several individuals, a susceptibility which would also tend to promote the success of the second vaccination. That this is the true explanation of the apparent anomaly seems probable from observing that the same order is followed in the failures, both when the original vesicles were good and when they were bad. If this be so—and I do not desire to dwell much upon a difference in failures which, after all, is not very considerable—it may become a subject for further inquiry whether the presence of four or five scars upon an arm (probably the result of as many punctures) may not sometimes be an indication of greater proclivity to suffer from the virus of smallpox introduced into the system than the presence of two or three cicatrices. This is quite a different thing from the proclivity to suffer from natural contagion, as I have insisted before. There is a fact observable in this

table which tends to confirm the inference from Mr. Marson's table respecting the relative severity with which smallpox is apt to attack persons with one or multiple scars. And it is this—that in those with one scar, who were revaccinated with success, a complete and perfect result was obtained more frequently rather than a modified result, a thing that is not observed in any of the succeeding series from two scars upwards; for in the latter either the proportion of perfect and modified results was equal or the proportion of modified results was more or less in excess, most in excess with persons having eight scars and upwards.

What I submit I have now shown is this—that the amount of protection gained from vaccination against casual contagion of smallpox, so far as the local phenomena indicate its influence, is not dependent upon the character (size or perfection) of the vesicles produced, but rather is connected with their number, with their quantity: but that the immunity from suffering from the disease, when once the virus gains access to the system, whether by inoculation (probably) or by way of natural contagion, is dependent somehow both upon the number and perfection of the vesicles produced by vaccination. In considering, then, the influence exerted by number and quality combined, I may confine myself to the simple question of severity of the smallpox attack when it does occur. The general result is furnished in the abstract of one of Marson's tables in the Appendix (Table 8). Without calculating per-centages a mere inspection of the gross numbers shows how in every series, with one, two, three, or four and more cicatrices respectively, the proportion of "modified" cases is greater when the cicatrices were good (perfect vesicles) than when they were indifferent (poor vesicles). And the same tale is told by the per-centages of fatality. Thus, the protection against the fatal severity of an attack which may happen

to occur is nearly three times greater when there was one good scar than when the single scar was of indifferent character; about two and a half times greater when there were two good scars than when there were two bad scars; and, again, greater when there were three or four good scars than when there were three or four bad scars. The order of protection as shown by the table seems to be this:

Number of vesicles.	Quality.	Fatality per cent. of smallpox.
4 or more	Good	0·99
„ „	Indifferent	0·00
3 „	Good	1·63
„ „	Indifferent	2·32
2 „	Good	2·68
1 „	Good	4·23
2 „	Indifferent	7·29
1 „	Indifferent	11·95

The Wirtemberg revaccinations would appear at first sight to indicate the contrary inference to that furnished by a consideration of Mr. Marson's table, inasmuch as, taking each series of persons with one, two, &c., good and bad scars, the failures and modified successes were proportionately greater in those whose arms presented bad scars than in those whose arms exhibited good scars. But this apparent contradiction is open to an explanation, which this is not the place to give.

3. *As to the constitutional disturbance.* I have now to show that *the constitutional disturbance accompanying the vaccine disease is one of the elements of the protection afforded by the operation.*

In maintaining an argument to prove the influence exerted by the constitutional disturbance—the “vaccinal fever”—I lie under this disadvantage, that, unlike the local affection, it leaves behind it no memory, no evidence of its severity,

unless, as I presume to hold, the character of the scars and their number can be accepted *in some degree* as an index of it. The areola fades away as the eruption dies off, and the fever is equally forgotten. Now, I think there is reason for believing that the perfection of the local disease may be thus viewed. Dr. Jenner himself appears to have noticed that there was generally some connection between the constitutional disturbance in persons infected from the cow, and the number and perfection of the pustules produced.* Ordinary and daily experience also attests that such is the case, and especially, that when the pustules are large and well developed, the areola (one of the marks of constitutional disturbance) is ordinarily better marked than when the pustules are small and imperfect in their character. *A priori* and analogical reasoning would lead us to a similar inference. Although it may fairly be inferred from the facts I have adduced, that the greater protective power of multiple and complete than of single and incomplete vaccine pocks may be, in part, dependent upon their local operation, whether by elimination or otherwise ; yet, when we consider that vaccinia is a constitutional (exanthematous) disease, marked not only by a local eruption but by more or less systemic disturbance ; and that the disease against which it serves as a protection is its analogue in this respect, we shall naturally be disposed to consider that the perfection of the former, as a constitutional disease is, after all, the *chief*—I guard myself against saying the only—matter of prophylactic importance. I think there is every reason to believe that the chief value which is to be attached to number and completeness as respects the vaccine vesicles rests on these conditions being a guarantee that the constitutional revolution which vaccinia is capable of bringing about has been satisfactorily undergone. As soon should I dissociate in my mind the prophylactic influence of

* 'Further Observations,' &c., 1799, p. 38.

the eruption and fever of smallpox.* As a rule, the severity of the initiatory fever of smallpox is greater, the constitutional disturbance more alarming, the more severe the subsequent local phenomena are about to become. Not that this rule is without exception, inasmuch as it has sometimes happened that the constitutional movement has been such that the medical observer has been led to anticipate a severe eruption, when a discrete or modified eruption is that which actually did succeed. I have already shown that a second attack of smallpox, as it has been observed in the Smallpox Hospital, was a milder disease on the whole, when occurring after the natural than after inoculated smallpox.

But, putting all such arguments aside, there are some direct proofs of the importance of the constitutional element of vaccinia, as respects the protection that it affords against

* I am anxious not to be misunderstood here. I do not place the perfection of the pock and the perfection of the vaccinal fever at all in any mutual relation of cause and effect. I hold that each is the result, independently of the other, of the vaccine virus, and that since for the most part when the virus operates efficiently it produces both good local and good constitutional results, and when it acts inefficiently it produces imperfect local and imperfect constitutional results, the local phenomena become an index of the constitutional operation of the virus. Two facts would lead us especially to discard from our minds any *causative* connection between the pock and the vaccinal fever. One of these is a phenomenon observed in double vaccinations with two vaccine viruses of different degrees of energy. When a powerful virus is inserted into one arm and a less powerful virus into the other (as in the comparative experiments of Bousquet and Steinbrenner, to which I shall refer hereafter), the more powerful virus produces at the punctured spot a very superior local result and strong vaccinal fever; yet all this does not influence the progress of the pock resulting from the weaker virus, for it becomes no better developed, and its course is in no way different from what it would have been had that virus alone been introduced into the system. The other fact is that in the Hindoo, as observed by Stewart, although the vesicle, from the different structure of the skin probably, is less magnificent than in the European, the constitutional impression is equally sufficient to serve as a protection against smallpox.

smallpox. Dr. Jenner, very early in his inquiries, recognised this fact. I have already referred to one of his observations—the case of Elizabeth Sarsenet, who, although she had cow-pox by contagion from the cow, nevertheless subsequently suffered from an attack of smallpox. In this case it was noted that there was no general indisposition; and, in order to show that this general disturbance of the system is of importance, Dr. Jenner places in antithesis to it the history of another girl upon the same farm who had cow-pox at the same time.* In this second case there was general indisposition, which lasted for a day or two; and, although the former girl was not protected, this one was, as is proved by several fruitless attempts having been made to impart smallpox to her by inoculation.

But of all the facts which could be cited to prove the influence which the vaccinal fever exercises in protecting against smallpox, perhaps the most remarkable are those quoted by M. Bousquet.† An infant was vaccinated by Dr. Pistorio: no pock whatever followed, but, on the eighth day, when the evidences of constitutional invasion should have appeared, had there been a pock, febrile disturbance was observed, which lasted for a week. This child was revaccinated, but on this occasion not only were there no vesicles, but also there was no fever. M. Petiet vaccinated a child who had been vaccinated the year before without success. In this case also no pock arose, but at the end of eight days a febrile commotion was observed, which lasted thirty-six hours. Some redness and swelling on the arm disappeared at the same time with the fever. On three several occasions attempts were made to revaccinate this child, but without success, and the subsequent inoculation of smallpox virus was equally fruitless. Similar results followed in cases

* Jenner, *op. cit.*, p. 71.

† *Op. cit.*, p. 311.

which Bousquet quotes as having occurred under the observation of MM. Raynal, Sauvairc, and Castéra. But the most remarkable facts of all are a series of events which are reported to have happened in the General Hospital at Nantes. It was at a time (July, 1825) when smallpox was epidemic, and cases of the disease were in the wards. Mr. Tréluyer vaccinated on the occasion five children: no vesicles arose, but on the second day they suffered from headache and anorexia, and on the third there was distinct febrile disturbance. M. Cormerais revaccinated them, but on this occasion not only were there no vesicles produced, but neither was there fever or any other result of the operation whatever. M. Cormerais then vaccinated five other children, and the result was the same as that obtained in the case of the first five—no pock, only fever. M. Barthelemy now attempted the vaccination of the first five, but without result; and he in his turn vaccinated a third series of five children, producing merely the same effect as was produced in the two former—fever, but no pocks. M. Duparc now took his place, and succeeded no better; he also produced fever, but no pocks. Recourse was now had to M. Rouillard, the Conservator of the Dépôt of Vaccine for the Department of the Loire Inferieure, and his vaccinations at the hospital were similar in result to those of all his predecessors. So here was a series of sixty subjects varying in age from ten to twenty-four years, in whom the vaccination produced nothing but a certain amount of constitutional disturbance, but no pocks, and in whom revaccination produced absolutely no result at all, not even the fever induced by the primary vaccination. Nevertheless, these subjects passed several months in the hospital, exposed to all the chances of contagion, mixing with the smallpox patients and partaking in their amusements without contracting the disease—all except two, in whom no effect at all was produced, either local or

general. But a further experiment was made. M. Tréluyer inoculated five of those in whom the general symptoms had been most marked with smallpox virus without producing an attack of the disease. Nevertheless, they did not pass wholly unaffected: for on the day following constitutional symptoms were noticed, cutis anserina, headache, nausea, vomiting, epigastric tenderness, diarrhœa, &c.: these symptoms lasted for eight days, and then all was over. Now, although it is very evident that there was present, at the time when these vaccinations were performed, some general condition (atmospheric or otherwise) which modified the result both of the vaccination and of the subsequent smallpox inoculation, in the instance of the former withholding the local operation of the vaccine virus, and in both the former and the latter bringing about febrile disturbance as early as the second day from puncture; yet one cannot avoid observing that the vaccinal fever really did in these persons, although there was no pock, afford a certain decided amount of protection against smallpox contagion. Either this is true, or the narration is a fable: the latter alternative I presume will scarcely be maintained. I hold it is clear that, in these instances that have been mentioned, the vaccinal fever stood alone as the representative of the vaccine disease, and afforded absolute protection against the reception of the virus by ordinary contagion; but in the Nantes series only a doubtful protection against the development of the virus introduced by inoculation into the system.

We are now prepared to consider the question,

How soon after the performance of Vaccination may the subject of it be considered protected against Smallpox?

If it be true that the perfection of the vaccine disease as an exanthematous malady is essential to the complete protection (allowing of course for a class of exceptional cases such as are met with in the instance of all maladies), we should expect that perfect protection against the virus of smallpox would not be obtained until the revolution of the system, brought about by the disease which is its substitute, has been completed. Again, as this revolution is not a sudden event, but as it commences from the time that the first local manifestation of the operation of the vaccine virus appears, and terminates at the time when the constitutional disturbance comes to an end, we should perhaps expect to find that there will be observed, between the one period and the other, grades or degrees of protection. And so, too, it will be no matter of surprise that smallpox should happen, and even run a severe course, in a person who has very recently been vaccinated. In such a case the course of the smallpox and of the vaccinia will depend upon which of the two first gains possession of the system, which is first in operation; for the disease which is first on the field will occupy it in preference to the other. During the Marseilles epidemic of 1828 we learn from M. Bousquet that sixteen persons died of smallpox during the period of the development of the vaccine. In the view of the case which I have just briefly outlined, this fact cannot fairly be used as an argument against the protective power of the latter.

The period after vaccination at which protection is obtained and the degree of protection afforded at various periods, may be judged of by reference to the several tests already laid

down, viz., inoculation, exposure to smallpox contagion, and revaccination. We will take each of these separately.

1. The result obtained *by smallpox inoculation* at various periods after the operation.

Some of the earliest experiments, by way of smallpox inoculation within a few days of vaccination, were made by Dr. Woodville.* He tells us that in twenty-eight instances he inoculated a mixture of the vaccine and variolous virus into the same puncture. The result was that, in about half the cases he obtained the vaccine vesicle, and in half the smallpox pustule, which ran its course as if it had been inoculated singly, and was followed as usual by the secondary eruption. It is clear from this that the two viruses do not antagonise the operation of each other by mutual destruction. When the two viruses are inoculated on the same day into different points, it has been observed† that the two diseases run their course quite independently the one of the other, each preserving the characters belonging to itself. The closest proximity of the vaccine and variolous pocks fails to cause any essential modification. Thus, Willan informs us that, when the two viruses are inoculated into points so close together that the pustules formed run into one as they enlarge, by inoculating afterwards from one side of the compound pustule, the vaccine disease will be communicated alone, while, on inoculating from the other side, the smallpox will be communicated alone, with its characteristic secondary eruption.

Cases 5 to 10 in Woodville's 'Reports' are instances of variolous inoculation performed within the first few days after vaccination.

In Case 5, the inoculation with smallpox virus was performed *on the third day* from vaccination. The result was

* 'Reports on Inoculations for Cowpox, 1799,' p. 25, *et seq.*

† Willan, 'Diseases of London,' p. 314.

the occurrence of smallpox, the secondary pustules of which appeared on the seventeenth day, the fourteenth from the inoculation.

The other five inoculations (Cases 6 to 10) were performed *on the fifth day of vaccination*.

In Case 6, an attack of ordinary discrete smallpox followed, the earliest pustules appearing on the thirteenth day of vaccination, the eighth of inoculation, when the cow-pox vesicles were scabbing.

In Case 7, both the primary eruptions proceeded in the ordinary way to maturation, and on the seventeenth day of vaccination, the twelfth of inoculation, both began to scab, so that they could not be distinguished the one from the other. In this instance, *no secondary smallpox eruption* appeared, the operation of the smallpox virus being thus in great measure superseded and rendered abortive.

In Case 8, the smallpox proceeded unhindered; and on the fourteenth day from vaccination, the ninth of inoculation, the secondary eruption commenced, about 300 pocks being produced.

In Case 9, the secondary smallpox eruption also appeared, the earliest pocks being observed on the twelfth day, the seventh from inoculation. But only two or three pocks altogether came out.

In Case 10, that of a child seven years old, the primary effect of the variolous inoculation was imperfect, and no secondary eruption took place.

Cases 15, 20, and 21, in Dr. Woodville's series are cases in which variolous inoculation was performed *on the fourteenth, thirteenth, and ninth, days of vaccination* respectively, and in none of the three was any effect produced.

Dr. Willan* held that, when the interval between the two inoculations did not exceed a week, the vaccinia and small-

* 'On Vaccine Inoculation,' 1806, p. 2.

pox each pursued its course without much variation ; but that, when the variolous matter was inserted on the ninth day after vaccination, its action seemed to be wholly precluded.

Bousquet supplies us in his work* with the results obtained by M. Mongenot at the Hôpital des Enfants Malades. M. Mongenot inoculated with smallpox virus four infants *on the fifth day of vaccination*. In two, there was a *partially successful* result ; in the other two *no result* at all. Thus, in the first of the four, he obtained four elevations, which, at the end of six days, presented a variolous appearance but were hard, horny, and contained no fluid. An unsuccessful attempt was made to inoculate another child from them. In the second case, the points of puncture became raised the next day, but, by the seventh day, the effect of the inoculation had all passed off. In the two remaining cases (although there was only a single vaccine vesicle upon the arm), the inoculation on the fifth day remained without result.

Two infants were inoculated *on the sixth day of vaccination*. In these the only effect produced was some irregular local irritation, but *no eruption*.

An infant was inoculated *on the seventh day of vaccination*. Again, only some local irritation resulted, which was all passed off at the end of five days.

M. Jadelot went a step further and inoculated three children *on the eighth day of vaccination*. In these, no result at all, not even any local irritation followed the insertion of the smallpox virus.

The inoculation experiments made by Sacco† do not correspond precisely with the above results, but are more in unison with the original observations of Woodville. He found that when he made his variolous inoculation *from the first to the fifth day* from vaccination, smallpox pustules

* 'Traité de Vaccine,' p. 286.

† Quoted by Steinbrenner, op. cit., p. 629.

appeared on the seventh, eighth, ninth, tenth, or eleventh day, which ran their course, merely complicating the vaccine disease. Children inoculated *on the sixth or seventh day* never had a general eruption; in some of them, there only occurred some inflammation at the points of inoculation, while, in a few, a local pustule formed which very rapidly dried up. When the inoculations were made *from the eighth to the eleventh day*, little or no effect was produced; only now and then a small local pustule appeared which almost immediately dried up. When the inoculation was made *from the eleventh to the thirteenth day*, there was generally no local change observed of any kind.

I have mentioned the observations of MM. Mongenot and Jadelot made in connection with the Central Committee of the French Academy. M. Marin, a member of the same committee, inoculated thirteen vaccinated subjects *on the ninth day*, making three punctures on each arm, and obtained no result except a slight local irritation. On the other hand, the experience of the committee was that, when the inoculation was performed from one to four days after the vaccination, the two eruptions were developed regularly and simultaneously, the one in no way influencing the other either by acceleration or retardation.

What then is the inference to be drawn from all these observations? Surely this; that so long as the vaccine disease is in progress, and not yet fully developed, the system is still unprotected against the operation of the smallpox virus. The question of "receptivity," strictly speaking, cannot arise, for the smallpox virus was in these experiments actually introduced: but it is simply one of "capacity for developing" the smallpox virus. Up to the fifth day, that is, up to the day after that on which the vesicle begins to rise, no protection is obtained, or none that can in any way be relied upon. On the sixth and seventh days there is some amount

of protection: but it is not complete until the eighth day, when the vaccinal fever, the constitutional effect of the vaccination, is first manifested,—when, for the first time, the vaccine disease can be regarded as fully developed. Up to this day the blood has been undergoing a slow revolution: on or about this day this revolution is completed, the nervous system is impressed, and the areola and vaccinal fever (just as in smallpox the rigors and other disturbances ushering in the secondary eruption) appear.

But if vaccination affords no full protection against the operation of smallpox virus until so late a period of the eruption as the eighth day, and no protection whatever until the sixth day, it follows that no protection is afforded if the introduction of the vaccine virus follows the introduction of the smallpox virus into the system. It is scarcely necessary to offer any proof of this, but I may refer to four of Woodville's inoculations. In Cases 11 and 12 of his series two adults were vaccinated on the day following smallpox inoculation. The eruptive fever commenced on the eighth day in one of them, and on the sixteenth day in the other, and both had an abundant crop of smallpox pustules. Cases 13 and 14 were boys in whom a similar experiment was performed: the secondary eruption appeared on the twelfth day in the one, and on the fourteenth day in the other. It is to be noted, although these cases are too few to draw any general conclusions from, that the secondary eruption was retarded beyond the ordinary period in three out of the four instances, and a similar occurrence was observed in Woodville's fifth case, where the inoculation was performed on the third day from vaccination. Still this is a thing which may occur quite independently of vaccination. Willan* relates a case where vaccination was performed as long as ten days after variolous inoculation, and a smallpox pustule

* 'On Vaccine Inoculation,' p. 6.

actually arose and matured within the border of the vaccine vesicle: both were possessed of their peculiar attributes; the one conveying smallpox, the other vaccinia by inoculation into other persons. This experiment, as well as those of Woodville, is the converse of the experiments previously detailed, and teaches the same lesson; inasmuch as the vaccine disease was shown capable of development, so long as the full constitutional operation of the smallpox virus had not been brought about. The conclusion that Willan arrived at from his experience is similar to that I have arrived at from the numerous observations I have described, namely, "that the two viruses do not interfere with each other's action while they are merely in progress, without having had their full operation. That vaccine inoculation is not a preventive with regard to smallpox until its course is completed, or at least till the inflammation round the pock be upon the decline."* The only modification to be made in this remark is to say, "until its course is complete," that is, until, as an exanthematous disease, the vaccinia has reached its full development.

2. The results of observation of *casual smallpox* as met with concurrently with vaccinia.

In estimating the influence exerted by a recent vaccination upon the reception and development of smallpox virus, received casually by the natural process of contagion, there is a point to be kept in mind which some observers have allowed to slip their memory, when drawing inferences from the facts which have come under their notice. This is the period of incubation of the virus. This period has been variously stated. For the most part, it lasts from twelve or fourteen days, that is to say, from the day of reception of the contagion to the first commencement of the eruption. Sometimes it appears that the period may be prolonged

* 'Diseases of London,' p. 316.

even to the extent of twenty days. During these days, however, the virus is not idle; it is operating, probably in the earlier days, in making its way from the surface upon which the virus has chanced to fall, into the blood; and, during the remainder, in preparing the blood for that final explosion, commencing with the disturbance of the nervous system which distinguishes the initiatory fever. According to the progress made by the virus, then, at the time that vaccination is performed, may we expect the smallpox to be arrested altogether or modified in its course by the vaccine virus introduced directly into the blood. Now, all the observations which are sufficiently accurate to be relied upon, tend to show that such is the case; that the earlier in the course of the vaccination the smallpox eruption appears, the less is its severity modified; and that, after the vaccine disease has run a certain course, smallpox contagion either cannot be received at all, or is incapable of producing its specific effect upon the economy. Bousquet puts the relation of the two viruses, smallpox received by contagion and vaccine introduced by the lancet, thus: "In the influence of vaccine upon smallpox or of smallpox upon vaccine, there is nothing direct or active or special. It is the consequence of the property they have of supplanting one another. The vaccine does not stop smallpox—it is smallpox which arrests itself in the presence of vaccine. . . . It is a right of preoccupation. So far from the two viruses destroying one another, they may be mixed, and, when inoculated together, two distinct eruptions are obtainable."*

The following are some of the facts which may be quoted in support of the view I have expressed, and which corresponds with that of Husson,† namely, "that the preservation produced by the vaccine disease, is to be dated from the

* 'Bull. de Thérap.,' t. xxxv, p. 351.

† 'Dict. des Sciences Médicales,' art. "Vaccine," t. lvi, p. 437.

moment that its specific constitutional impression manifests itself, that is, on the ninth or tenth day."

One of the earliest observations upon this subject is by Willan,* who briefly relates a case where casual smallpox appeared upon an adult female, six days after vaccination. In this case, two variolous pustules arose within the circumferences of the vaccine pock.

Adams,† in 1805, related the events which occurred in a family containing a brother and two sisters. The former had smallpox; and on the fourteenth day from that on which he sickened, the two sisters were vaccinated. The elder sister (probably not having received the contagion) escaped smallpox, but upon the younger some variolous pustules appeared *three days after the areola had been formed* around the vaccine pock. They died away, however, after a few days. In this case the vaccinia was tardy in its development, the areola not being formed until the eleventh day from vaccination. Probably the smallpox *contagion was received on or about the day of vaccination*, making allowance for the ordinary period of incubation, and the disease was to be regarded as modified in its progress.

During the Norwich epidemic, Cross‡ states that he met with smallpox coming out at all periods until near the termination of the vaccine disease; and convinced himself that this greatly mitigated the violence of the natural contagion, because none died out of about thirty cases in which the two diseases occurred together. In general he observed that, the later the period at which smallpox appeared, the more mild was it rendered. "The influence of the two diseases has been reciprocal. If the smallpox has appeared before the time that the areola should begin to form around

* 'Diseases of London,' 1800, p. 314.

† 'Med. and Physical Journal,' vol. xiv, p. 199.

‡ 'Op. cit., p. 45.

the vaccine pock, the latter has subsequently been altered in its character, losing its circular shape, becoming irregular, flatter, sometimes having its contents nearly absorbed, leaving an empty bag and ending in an imperfect thin scab, whilst the variolous eruption has proceeded in its regular course. But when the appearance of the variolous eruption has been deferred until the areola has formed around the vaccine pock, this has ended in a regular scab, and the variola has generally, though not uniformly, proved mild. . . . Appearing later than the ninth or tenth day from the vaccination, they have always, according to my experience, assumed the mild form, been in small number and lasted only three or four days." Husson, in accordance with his view, also places the period of protection against the development of casual smallpox at the ninth or tenth day. That is to say, he and Cross both agree that, although the contagion may have been received some days prior to vaccination, the *latter prevents the development of the disease so long as the incubative period extends beyond the ninth day.*

Still this rule does not appear to be absolute, inasmuch as Heim relates a number of cases of natural smallpox occurring with vaccinia, some of which were fatal, although the eruption did not appear until the eighth or twelfth day after vaccination. It is to be remarked here, however, that the mere *day* on which the smallpox eruption is due is of less importance than the *stage* that the vaccine disease has arrived at. In some children the vaccine runs a more tardy course than it does in others : and there are some persons who believe that the presence of the contagious virus of smallpox operating in the system, of itself tends to impart such tardiness to its development. In the Wirtemberg epidemics this occurrence of smallpox with vaccinia was met with twenty-four times in infants under one year of age. Of these twenty-four cases, seventeen were true smallpox, and seven were instances of

“varioloid.” The following table, representing the dates after vaccination on which the eruption appeared, and the result of the attack shows us this—that the true smallpox may appear even up to as late as the fifteenth day from vaccination (the contagion having been probably received about the same day as the vaccine virus was introduced, as in Adam’s case); but that, judging from the fatality, the disease was milder the later the period of the appearance of the smallpox. Out of the seven deaths five occurred in patients in whom it appeared prior to the fifth day of vaccination. One deficiency in this table is that it does not show whether any of the deaths, and especially those of patients attacked at a late period, resulted from superadded disease. If we assume that the day of the development of the vaccinal fever or full constitutional impression is the eighth day, then it would appear that smallpox, the contagion of which is already working in the system, may appear even as long as seven days thereafter. But it is open to question whether, in these late cases, the vaccine disease was as fully developed as it might have been.

Day of vaccination on which smallpox appeared.	True smallpox.	Died.	Varioloid.	Died.	Total.	Died.
1st Day	1	1	1	1
2nd „	1	...	1	...
3rd „	5	3	5	3
4th „	3	1	3	1
5th „	1	1	...
6th „	1	...	1	...	2	...
7th „	1	...	1	...	2	...
8th „	1	1	1	...	2	1
9th „	1	...	1	...	2	...
10th „	1	1	1	1
11th „	2	1	2	1
12th „	1	...	1	...
15th „	1	1	...
Sum	17	7	7	1	24	8

Heim also met with four instances of concurrence of the two diseases in adults varying from twenty to twenty-eight years of age.

In two of these, “varioid” appeared two days after vaccination.

In one, true smallpox appeared on the same day as the vaccinal eruption.

In one, a *second* attack of smallpox appeared on the third day from vaccination, and was fatal in three days.

Putting all these 28 cases together, what we find is this—that of the 14 cases in which the smallpox appeared before the fifth day, 10, or 71 per cent., were unmodified smallpox, and 4, or 29 per cent. were “varioid;” and out of the 14 which occurred at later periods of vaccination, 9, or 64 per cent., were unmodified, and 5, or 36 per cent., were “varioid.” The difference is not considerable. But, regarding

fatality as an index of severity of attack, we find that out of the 14 attacked prior to the fifth day, 6 died; while out of the 14 attacked from the fifth day onwards, only 3 died.

Next, taking the ninth day as that on which the vaccinal fever may be regarded as, in nearly all cases, fully established, we may compare the period prior to this day with that subsequent. We find then, that, out of the 28 cases, 21, or 75 per cent., occurred prior to the ninth day, and only 7, or 25 per cent., happened on and after that day. Again, of the 21 that happened prior to the ninth day, 14, or 66 per cent., were "unmodified," and 7, or 33 per cent., "varioid;" out of the 7 cases that happened on and after the ninth day, 5, or 71 per cent., were "unmodified;" the difference in this respect not being of much importance. Lastly, of the 21 prior to the ninth day, 7, or 66 per cent., died; while of the 7 that happened on and after the ninth day, 2 only, or 28 per cent., died.

M. Hérard* observed 18 cases of the simultaneous occurrence of smallpox and vaccinia, the smallpox appearing as follows:

On the	2nd day	2 cases.
"	3rd	"	2 "
"	4th	"	1 "
"	5th	"	1 "
"	7th	"	2 "
"	9th	"	2 "
"	10th	"	4 "
"	12th	"	1 "
"	14th	"	3 "

Now, here it is observable that in the greater number of the 18 cases the smallpox broke out on or after the ninth day. He does not distinguish between the cases of true smallpox and

* 'L'Union Médicale,' 1848, p. 428 *et seq.*

“varioid,” nor between those which were fatal, with reference to the days of outbreak. He tells us merely that 7 of the 18 died—a very large mortality, as Bousquet takes occasion to point out. Hérard distinctly states, however, that all these 7 were exceptional cases in this respect, that they happened in cachectic children worn down by previous disease; whereas the remaining 11 children who recovered were previously in good health.

Gaultier de Claubry saw the concurrence of vaccinia and smallpox twice: in one case, a child four years old, the eruption was discrete, and the child recovered; in the other, nine years old, it was confluent and fatal. In the correspondence transmitted to the Acad. de Méd., in 1846, this coincidence is stated to have happened thirteen times and more. Two of the cases were confluent, and one of them was fatal. Other writers, as M. Clérault and M. Legendre, have made collections of cases of a similar character. Out of 111 collected by the former twelve died, and out of fifty-six collected by the latter nine died.

Now, I must say, at once, that none of the observations above referred to give information sufficiently full and precise for the determination of all the points which it is desirable should be settled. Those of Heim are the nearest to what could be wished, but he fails to distinguish between the persons who died from the smallpox and those who died from the influence of concomitant conditions. Taking all the observations together, such as they are, the only inferences which can be drawn from them are the following:

1. That smallpox may break out on a vaccinated person up to as late as the fourteenth or fifteenth day with its natural characters (Heim and Hérard); that is, allowing for the period of incubation, smallpox contagion may be received on the day that the vaccination is performed, and,

probably, also a few days later, and proceed, notwithstanding the vaccination, to its full development.

2. Adding together the cases referred to by the authors I have mentioned, we get the following result as to fatality :

	Cases.	Deaths.
Cross	30	0
Heim	28	9
Hérard	18	7
Gautier de Claubry	2	1
Legendre	56	9
Clerault	111	12
Acad., 1846	13	1
	<hr/> 258	<hr/> 39

Now, this proportion of fatality is 15 per cent. It is high as smallpox goes in these vaccinated days, but it is low as smallpox used to proceed before the discovery of vaccination and now among unvaccinated persons. In fact, it is intermediate between the mortality of vaccinated persons admitted into the Smallpox Hospital here, 5·25 per cent., and that of the unvaccinated, 35·55 per cent. The inference to be drawn, I think, is that smallpox, when happening in the course of the vaccine disease, is, on the whole, less fatal than when it happens prior to vaccination, but more fatal than it is when it occurs at a period subsequent to the completion of the vaccine disease. The observation of Hérard as to the recovery of all who were not weakened by previous disease can have little weight, inasmuch as the fatality of smallpox among all classes must be dependent partly on previous debility, and we have no right to exclude the sickly in estimating the fatality of any particular group, when comparing it with that of any other group of cases in which they are not excluded.

3. I am disposed to gather from Heim's table that the fatality of smallpox is less in those in whom the smallpox breaks out late in the vaccine disease than in those in whom

it breaks out earlier. That according to the duration of the incubation of smallpox, at the time when the vaccination is performed, will be the influence which the vaccine disease will exert upon the fatality of the smallpox. It is, according to Hérard, a fruitless thing to vaccinate at all when the initiatory fever of the smallpox has commenced; it will only take prior to this happening.

4. Allowing for the period of incubation twelve or fourteen days, I do not know of any instance where the contagion of smallpox has been received after the fourth day, the rising of the vesicle. But as some authors hold that the incubation of the disease may be shorter than this, it may be safely affirmed that it cannot be received when the vaccinal fever is developed and the areola formed on the eighth or ninth day.

There is great need for renewed and accurate observation upon this subject, but it must be approached with an unprejudiced mind.

3. *The result of revaccination during the progress of the vaccine disease.*—This test, as I have before pointed out, is only a substitute for the inoculation test, and, like it, is calculated to indicate the capability of the system for developing the virus.

This test was adopted early in the history of vaccination by Mr. Bryce,* with a view to determine whether, in any individual case, the system had been sufficiently impressed by the vaccine disease to assure protection against smallpox. He says that he called to mind some observations which had been made, that in smallpox inoculation, if the inoculation were repeated daily until the fever occurred, the later punctures advanced more rapidly than the first had advanced, and even that the puncture last made, perhaps only twenty-four hours before, equalled in maturity that made eight or nine days

* Bryce, 'Practical Observations on the Inoculation of Cow-pox,' p 159.

previously. His book contains a number of experiments, showing that a similar law prevails with the vaccine disease. He found, for example (as in his third case), that, when he performed vaccination on the sixth day of primary vaccination, the vesicle rose rapidly, and that by the fourth day from puncture an areola was formed. When he vaccinated on the seventh day (Case 5), again, the vesicle appeared on the third and the areola on the fourth day. When he revaccinated on the eighth day (Case 4) he still succeeded, and the areola was formed on the sixth day. In all his cases he revaccinated before the areola was formed around the first vesicle, and found that the second vesicle pursued altogether a more rapid course than the first, arriving more quickly at maturity, and that both faded away about the same time. In one instance (Case 6) he vaccinated twice, first on the sixth day of primary vaccination, and then again on the seventh, but both were before the areola of the primary vesicle was formed. In the instance of the first revaccination the areola formed on its fifth day, the vesicle appearing on the fourth. In the instance of the second revaccination the vesicle appeared on the fifth day, but was very imperfect, and it was doubtful whether an areola was formed at all.

Mr. Hugo, of Crediton, is quoted by Willan as having shortly confirmed Bryce's observation; and he also found that, by revaccinating on the sixth day before the areola was formed, the vaccination took; that the vesicle pursued a rapid course, but was more diminutive than the primary vesicle.

Similar experiments were instituted by the Central Vaccine Committee of the French Academy and by Bousquet. The former failed to produce a second vaccine pock after the sixth day from the primary vaccination. Bosquet states that he never found the revaccination succeed after the fifth day.

Eichhorn, who performed also a number of similar revac-

inations, established positively that they never succeed after the areola has been formed.

I cannot pretend to reconcile the discrepancies of the above observations. Possibly the period at which the revaccination may be successfully performed depends upon conditions hitherto left out of consideration, such, for example, as the lymph used in vaccination and the number of primary vesicles. One thing, however, all agree in, namely, that the formation of the areola distinctly marks a day beyond which the reintroduction of vaccine virus is absolutely fruitless. If this should appear on the eighth day, then no second vaccination will succeed on that day; if on the ninth day, it will succeed even on the eighth. *The areola indicates the incapability of the system to undergo further impression.*

Let me then sum up the result of the application of these several tests by saying, that smallpox virus may probably be capable of being received into the system for some few days after vaccination, but most certainly not after the development of the vaccine fever and areola; further, that, being received (as by inoculation), it is incapable of producing any effect whatever, even a local one, after the vaccinal fever and areola are established; that, if it be received after the vesicle is formed, a local effect only is produced; but that if it be received into the system prior to the formation of the vesicle, the constitution is open to its operation. In all my discussions of this question I have carefully distinguished between the reception of the virus by some surface in casual contagion and the operation of the virus when received. Now, these two distinct processes make up what we call the *incubation* of casual smallpox. Strictly speaking, I imagine the "incubation" of the disease should be understood as the period during which the virus is operating in the system prior to the appearance of the eruption. In inoculation of smallpox (the virus being introduced at once) the eruption of

the general disease does not occur until about the eighth or ninth day. Why, then, should we believe that the virus introduced in any other way operates longer in the system before the eruption appears? If this be so, and eight or nine days be the period of *true* incubation, the days over and above, back to the day of contagion, must be occupied in the process of reception—in the passage of the virus into the blood. We can, perhaps, understand that this process may be somewhat easier than usual in some persons, somewhat more difficult in others; and hence that contagion, received upon the day of vaccination, may not result in the arrival of the virus into the blood until after the vaccine pock has formed; in this case, judging by the experiments of inoculation and revaccination, the disease, if it be not arrested, will probably be less severe or less perfect than if it were received earlier. Should the virus have arrived at the blood, either from earlier contagion or from more ready reception, prior to the formation of the vesicle, we may expect to find that the disease would not be mitigated in any way, but become developed just as it would had no vaccination been performed at all. Should it, however, not arrive at the blood until the areola is formed, either from late contagion or from difficult receptivity, we cannot, in the face of experiment, believe that the virus would develop itself at all. Hence, if I am correct in my reasoning, much of the influence of the vaccination upon the operation of casual contagion will depend, not merely upon the day of exposure to contagion, but also upon the readiness with which the virus in any individual finds its way into the blood. Are there any external marks by which this can be predicated? I do not know.

Is the protection against smallpox afforded by vaccination limited in duration?

Two main points have now amongst others been established. One of them is that, by the time the vaccine disease has completed its course, the system has lost its capacity, certainly, for developing the virus, and, so far as we can discover, the power of receiving into the system at all the virus conveyed to any surface by casual contagion. The other point is that, in a proportion of vaccinated persons, notwithstanding this first and immediate immunity, the power of receiving and the capacity for developing the virus when received into the system are more or less fully recovered. Now, the questions which arise are these :

1. Is the capacity for developing smallpox virus in the system always recovered at some time or other by vaccinated persons?

2. Is the power of receiving the virus of smallpox, as conveyed by casual contagion, always recovered by vaccinated persons?

3. Is the power of receiving the contagion or the capacity for developing the virus first recovered?

4. Is there any reason to believe that, in those persons in whom receptivity for the contagion of smallpox returns after vaccination, the return of the receptivity is gradual and due to a progressive weakening of the protective power of the vaccine disease by lapse of time?

5. Is there any reason to believe that, in the majority of vaccinated persons who exhibit a return of the capacity for developing in their system the virus of smallpox or its substitute, the vaccine virus, the return of capacity is gradual and due to a progressive weakening of the protective power of the vaccine disease by lapse of time?

Each of these questions may be discussed separately.

1. *Is the capacity for developing smallpox always recovered at some time or other by vaccinated persons?* This is much the same as asking whether, in *all* persons, it is found that at some period of life after vaccination inoculation of smallpox or vaccine virus is again capable of reproducing each its specific effect upon the system. And the answer is that, so far as we know, from the inoculations and revaccinations performed at various periods after vaccination, *this return of capacity of developing the virus does not take place in all persons.*

To take first the *inoculation test*. In Jenner's first work he relates a considerable number of cases in which persons who had had cow-pox by natural contagion, at periods extending up to forty years previously, were incapable of developing the smallpox virus which he introduced by inoculation. In respect of these cases, he distinctly says in a note,* "I have purposely selected several cases in which the disease had appeared at a very distant period to the experiments made with the variolous matter, to show that the change produced in the constitution is not affected by time." In 1832, Sacco's thesis† was published, in which he narrates experiments made by inoculation upon a number of persons vaccinated at various distant periods, namely, twelve boys vaccinated for two years, twelve persons vaccinated for twenty years, six vaccinated for twenty-two years, and two vaccinated for twenty-four years, none of which acquired from the inoculation either smallpox or the varioloid form of the complaint.

Several of Mr. Cross's correspondents also stated that they had attempted inoculation without success upon persons who had been vaccinated fifteen or twenty years previously; and, lastly, I may refer again to the unsuccessful inoculations

* Jenner, 'An Inquiry,' &c., p. 11.

† 'De Vaccinationis Necessitate,' 1832.

by Assistant-Surgeon Russel, which he made in India upon natives at various periods after vaccination up to twenty-one years.

I am compelled to have recourse to these older observations, inasmuch as smallpox inoculations are now almost universally prohibited by law.

But if inoculations of a tentative character cannot be performed, still the virus of the substitute for smallpox can be introduced; and, during many years past, *revaccinations* have been performed in many countries upon a large scale.* And the result has universally been that, although a large number of persons have been found susceptible of the influence of the virus, the revaccination produced no operation upon a very large proportion. Thus, during the five years 1831½-1835½, out of 14,384 persons who were revaccinated in the Wirtemberg army, the operation was wholly unsuccessful in 5919, or in 411·5 per 1000. The persons revaccinated were of various ages, from under twenty to over thirty years, and even over forty years of age. In the civil revaccinations in various parts of the kingdom of Wirtemberg† from 1831 to 1836, of people of all ages, 29,864 persons underwent the operation; and of these, 9142, or 306 per 1000, without success. Of 23,945 revaccinations performed in the Danish army (1843-5, and 1847),‡ 4241, or 177 per 1000, were complete failures, although most of them were submitted to a second trial. Of 1050 revaccinations performed in the Brunswick army (1844), 418, or 398 per 1000, failed. In 20,483 revaccinations in the Baden army there were 347 per 1000 failures. In the British

* Estlin states that several persons whom he found in 1838 affected with cow-pox from contagion on a farm in Gloucestershire had all been vaccinated, and one of them by Jenner himself. 'Med. Gazette,' vol. xxii, p. 977.

† Table; see Steinbrenner, op. cit., p. 839.

‡ 'Papers,' &c., p. 34.

army* revaccination was commenced in 1858. Among the soldiers already in the forces the failures during the seven years 1858-64 appear to have averaged about 311 per 1000 trials, and, upon the recruits, about 292·7 per 1000.

The only conclusion I am drawing now from these statements is, that there exist a large number of persons in whom both inoculation and revaccination fail to elicit any evidence that susceptibility to the development of the smallpox virus or its substitute has returned after the lapse of many years, even up to forty years, and more, after primary vaccination. What this proportion exactly is I do not think the statements I have quoted are sufficiently precise to determine. In the case of the revaccinations especially we must be guarded in our conclusions, since it has been found that, in a proportion of cases, a second attempt at revaccination will succeed where the first has failed, and that something of success or non-success may be due to the kind of lymph used, its source, and the number of punctures made. The lowest proportion of complete failures that I have quoted was that observed in the Danish army, 177 per 1000, and here a second trial was mostly made.

2. *Is the power of receiving the virus of smallpox as conveyed by casual contagion always recovered by vaccinated persons?* That is to say, do we find that all vaccinated persons, exposed freely to contagion of smallpox, are equally liable to take the disease? Of course, the reply to this question will be, in a degree, a reply to the preceding, because each case of *reception* by contagion is only shown to be such by the development of the disease, and development of the virus can only occur where there is a capacity for development. I am only concerned here with persons who have

* The results of revaccination are published annually in the 'Statistical, Sanitary and Medical Reports of the Army Medical Department.'

been vaccinated a long time—some years. And if we find that, in any instances, a primary successful vaccination has proved protective in persons freely exposed—as freely exposed as persons are known to have been who have taken smallpox after vaccination—our reply must be in the negative again. Hence, if we find vaccinated persons exposed to contagion by living in the same house with smallpox patients, in the same room or family, nursing smallpox patients, and so on, and yet not catching the disease, especially when others similarly situated in the same house or family with them do take the disease, all that is necessary to give a negative reply is provided. Now, we have only to look back to a former page of this essay to discover instances of these occurrences. For example, there is Mr. Johnson's series of cases (p. 45), where, in one family, a brother, vaccinated, took the disease from those in the house, while in another a wife, vaccinated, failed to take it from her husband. There are the 347 vaccinated medical men polled by Dr. Seaton, of whom 12·6 per cent. only took the disease, to which they had been frequently and closely in relation, while 87·4 per cent. did not receive it, although similarly exposed. There are the 60 medical men in the Medical Society of London, all of whom, being vaccinated, had failed to receive the disease with the bare exception of 4 or 5, although, probably, all had been equally exposed. There are 17 persons mentioned by Cross who escaped smallpox, though vaccinated from ten to twenty years before, and although living in the very rooms occupied by smallpox patients. There is the almost complete escape of the vaccinated poor persons in 77 families, where, during the Norwich epidemic, these persons herded closely with the sick; and in order to give an additional recent example, I may quote from a report of the French Committee.* An English family consisted of a father and mother vaccinated

* 'Rapport sur les Vaccinations en France pendant 1860,' p. 92.

many years previously (*anciennement vaccinés*) and ten unvaccinated children. One of the latter was attacked with smallpox, and in less than a month the nine others. Five, aged from eleven to twenty-two years, died. "The father and mother were respected." Now, there can be no question in this case, I presume, either of duration of exposure to contagion, which must have lasted two months or so, nor yet of closeness of communication, nor yet of concentration of the contagion. But for all this the father and mother remained protected through all. I need not go on further adducing proof of the fact that a primary vaccination may be sufficient to protect even under close exposure to contagion many years after the operation has been performed. It is almost enough for my argument to show that it is still efficient as a protection up to forty years of age, for this includes the periods during which the liability to smallpox is naturally greatest.

3. *Is the power of receiving the virus by contagion into the system or the capacity for developing the virus in the system first recovered?* This question involves a previous one. I have all along been keeping distinct the power of reception—true "receptivity"—and the capacity for development of the virus: have I right on my side in doing this? The only thing which can give me this right would be the discovery that persons may be capable of developing the virus either of smallpox or its substitute, when introduced into the system, yet have resisted the invasion of the disease, notwithstanding exposure to contagion. Now, here comes up again Dr. Gregory's case of the old lady at Salisbury who, in the inoculation days, brought up a large family without taking smallpox, yet at the age of eighty-one was successfully inoculated, and this, too, at a period of life far from that at which the liability to take smallpox is greatest. Then, again, there are the instances of the medical men which I have

already quoted (that of Heim being the most striking), where very frequent and close exposure to contagion had failed to give smallpox; and yet that they were capable of developing the virus was proved by their successful vaccination. Lastly, there are the successes, complete or modified, which have been observed over and over again in persons who have been vaccinated after suffering from smallpox, many of whom must at various periods of their lives have been subjected to a second contagion. I need only mention a single example, namely, what was observed in the vaccination performed in the Wirtemberg army, where, out of 266 persons with marks of smallpox, vaccination was successful in 567·6 per cent. It cannot be believed that these persons—some of them, at any rate—had not, in the course of the years succeeding smallpox, have been more or less exposed to the contagion of the malady.

The question, then, now is, whether there are any facts to lead us, in determining whether capacity for developing the disease or the power of receiving it into the system through some surface of the body is recovered soonest. Now, the materials for a reply may be found in a comparison of the proportion of persons who, having been vaccinated, have in a contagious atmosphere taken the disease, and that of persons who, having been vaccinated, are found to be capable of developing the vaccine virus on revaccination. The first datum is given by the Marseilles epidemic of 1828. For the second we will adopt the highest of the successes of revaccination I have quoted, viz., that afforded by the Danish army.

	Per cent. of vaccinated persons.	Per cent. of persons who had smallpox before.
Attacked by smallpox during the Marseilles epidemic	6.6	1.00
Success of revaccination, Danish army	82.2	(not distinguished)
Attacked by smallpox at Prague during twenty-one years	0.27	(not distinguished)
Success of revaccination, Wirtem- berg army (normal cicatrices)	59.09	56.7

We find here, then, that the revaccinations in the Danish army were successful more than twelve times oftener in developing vaccine in vaccinated persons, or those who had had smallpox, than an extensively diffused contagion was in imparting smallpox to vaccinated persons during the epidemic season of 1828 in Marseilles; and more than 300 times more successful than the ordinary smallpox contagion to which everybody is more or less exposed at some time of his life in the course of twenty-one years, such as must have been the case with the vaccinated people in Prague.

But then the question arises whether this difference in the readiness to develop the virus, when introduced, and the readiness to receive it by contagion, is not met with to the same extent among unvaccinated persons as a natural condition of things. And I think the conclusion which we must arrive at is that such is not the case. Failures of smallpox inoculation in unvaccinated persons are rare events; they are estimated at about two per cent. of the attempts. Woodville found the proportion of failures at the Smallpox and Inoculation Hospital to be one in sixty, or about 1.6 per cent. That is to say, 98.4 per cent. of inoculated persons develop the disease, and are thus shown capable of the process. Now, during the Marseilles epidemic of 1828 there were 8000 persons unprotected either by previous smallpox or vaccination, and of these 4000, or 50 per cent., suffered

from the epidemic contagion. That is to say, the number of those who were capable of developing the disease is nearly twice as large as those who appear to be capable of receiving the contagion when they are exposed to it.

The inference I draw is this :—1. That *naturally* the power of receiving contagion of smallpox is about half that of developing the virus when received. 2. That among *vaccinated* persons (at various periods after vaccination) the power of receiving the disease is more than twelve times less than that of the power of developing the virus when introduced; and hence that I am justified in believing that the process of vaccination does not only affect the capability of developing the virus but also of receiving it, in regarding these two things as distinct, and in making use of the argument I have employed to show that the power of receiving the disease by contagion after vaccination is one which is recovered much more tardily than that of developing the smallpox virus or its substitute when admitted into the system. A corollary to the last inference is that the success of revaccination is not absolute proof that a person is not previously protected against the reception of smallpox contagion. Only about one in twelve of those in whom a (repeated) revaccination succeeds is liable to catch the disease, or, putting the return of capacity for developing the virus aside altogether, vaccinated persons are six times less likely to receive the contagion by the natural method into their blood than unvaccinated persons are.

4. *Is there any reason to believe that, in those persons in whom receptivity for the contagion of smallpox returns after vaccination, the return of the receptivity is gradual and due to a progressive weakening of the protective power of the vaccine disease by lapse of time?* Tables 13,^a 14, 15, and 16, in the Appendix, will assist us in replying to this question. Table 13 contains the experience of the Smallpox Hospital

during the epidemic season of 1838. In that year only five vaccinated children under ten years of age were admitted, and not one under five years; at the ages from ten to fifteen years twenty-five were admitted, and at the ages from fifteen to twenty years as many as ninety. Here is a progressive increase in the number of admissions in the successive quinquennials of age. Confining our attention to the first three quinquennials, we find a true progression in 0, 5, 25 cases. And that the progressive increase was really due to gradual weakening of protective power, is to be inferred from the fact that, apart from previous vaccination, no such progressive increase was observed. The numbers of unvaccinated children admitted in the same year formed a diminishing series in the successive quinquennials of age, namely, forty-two, thirty-seven, thirty. Again, Table 14 is constructed from two tables furnished by Mr. Marson on which I have calculated the per-centages of fatality. A similar fact is brought to light on examining this table. In the first quinquennial of age seven vaccinated children were admitted, in the second fifty-six, and in the third 206; whereas, among the unvaccinated children, we find a diminishing series 356, 334, 270. Cross's table, again (Table 15), shows a diminishing series with advancing years from birth to fifteen years of age among unprotected children. Table 16 shows the number of cases of post-vaccinal smallpox observed by Heim at Wirtemberg from 1831 to 1836, and here, as in Gregory's and Marson's tables, we see a progressively increasing series from birth to fifteen years of age. In all these several observations the progression is decided and unmistakable, and, as it is utterly opposed to the natural condition of things in the unvaccinated, can only be attributed to the gradual loss of the protection conferred by the vaccination against the reception into the system of the contagion of smallpox.

This relates to the period from birth to early puberty. But

can the loss of protective power be traced further? Does the progressive loss of protection continue, and is the rate of the progression the same after fifteen years of age as during the first three quinquenniads of age? If we merely look to the number of cases of post-vaccinal smallpox occurring during the following quinquenniads of age, we shall find that they continue to increase after fifteen years of age up to the age of twenty-five years, and that from fifteen years to twenty years of age there occurs a very remarkable increase in the number of cases. This is observed in Gregory's, Marson's, and Heim's tables alike. After twenty-five years of age (in Heim's after twenty) the numbers rapidly lessen as one quinquenniad succeeds another. But are we from this to conclude that the relation which the successive numbers bear to one another exhibits faithfully the loss of protective power of the vaccine disease? If so we should have to conclude that from fifteen to twenty a loss of protective power happened greater even than between ten and fifteen years of age, that the loss continued up to twenty-five years (as would appear from Gregory's and Marson's numbers), and then that a sudden and remarkable recovery of the protective power took place, which became greater and greater as one period of age succeeded another. But this would be a monstrous inference, because it would involve a return of protective power absolutely lost, without any cause for the return. On the contrary, I think I shall be able to show that there occurs no such great loss of protective power after the age of early puberty, but still that a continued loss does go on, but at a slower rate of progression, up to an advanced period of life. And I shall attempt to do this by calling attention to the numbers of admissions of unvaccinated persons at the several quinquenniads and decenniads of age, and comparing them and their variations, as they appear in the series upon Marson's table, with the numbers of cases of vaccinated

persons admitted at the same ages. I shall show that there is an interfering cause operating in the series of vaccinated admissions—a cause which determines the receptivity equally in vaccinated and unvaccinated, and that cause is age, or, to speak more correctly, the constitutional peculiarities pertaining to various ages. But I shall show that, in the instance of the vaccinated, this cause alone will not account for the variations of admissions observed, but that there is room for the inference that, notwithstanding first appearances, a progressive loss of vaccine protection goes on, but at a diminishing rate, up to a late period of life.

We will first of all examine Table 14, where Mr. Marson deals with large numbers, and we will take one quinquenniad of age after another, and see if we can trace out the operation of these two causes of variation in the smallpox admissions of vaccinated persons over fifteen years of age. In the following table I have arranged the vaccinated and unvaccinated persons admitted according to ages, representing the total number of each as 1000, adding a column of the Wirtemberg cases represented in the same manner:—

Smallpox Hospital cases.				Wurtemberg cases.
	Unvaccinated per 1000 cases.	Vaccinated per 1000 cases.	Difference of vaccinated from unvaccinated.	Vaccinated per 1000 cases.
Under 5 years...	134	2	- 132	38
5 to 10 „ ...	126	18	- 108	64
10 „ 15 „ ...	102	67	- 35	176
15 „ 20 „ ...	215	280	+ 65	261
20 „ 25 „ ...	252	342	+ 90	227
25 „ 30 „ ...	102	170	+ 68	163
30 „ 40 „ ...	58	101	+ 43	71
40 „ 50 „ ...	7	20	+ 13	(30—35 yrs.)
50 „ 60 „ ...	3	0.6	- 2.4	
60 „ 70 „ ...	0.8			
70 „ 80 „ ...	0.4			
80 „ 90 „ ...	0.4			

Beginning with the quinquenniad fifteen to twenty, we find an increase of cases, both in the vaccinated and unvaccinated column. In the latter the number of cases was rather more than twice what it was in the quinquenniad ten to fifteen years, but in the former it was about four and a quarter times as great. I apprehend that the unvaccinated column exhibits the natural increase of receptivity due to the age, and the difference of the rate of increase among the vaccinated exhibits the further or additional influence of the loss of protective power of the vaccination. Next let us take the quinquenniad twenty to twenty-five, and again we find an increase in the number of admissions, both of vaccinated and unvaccinated, above the number of the preceding quinquenniad. Here, then, again there is an increase due to age; in the unvaccinated column the extent of increase is a little under one fifth; this is the natural increase. In the vaccinated column the extent of increase is a little over one fourth; the difference is the effect of the progressing loss of power of the

vaccine disease. The loss of power is going on now at a very much slower rate than in the preceding quinquennials; but it is going on. The quinquennial twenty-five to thirty exhibits, both in the column of vaccinated and unvaccinated, a diminished number of admissions, showing a naturally lowered receptivity for contagion. Among the unvaccinated the reduction was to the extent of about three fifths from the number of admissions in the preceding quinquennial; this is the natural reduction of receptivity due to advancing age. Among the vaccinated the reduction was not so great; it was only to the extent of one half. The difference between three fifths and one half represents the continued loss of protective influence of the vaccine. In the next decennial, thirty to forty years, there was a further diminution in the number of admissions, both of vaccinated and unvaccinated. Among the unvaccinated the reduction was to the extent of rather more than two fifths; this is the natural reduction, then, due to age. Among the vaccinated the reduction was about two fifths; so that the difference, though still observable, between the extent of reduction in the vaccinated and unvaccinated series is now very small; but it is sufficient to mark the fact that a loss of protective power has still been going on, but at a greatly lessened rate. After the age of forty the total numbers upon Marson's table become so small that the precise relation between the two columns cannot be relied upon. But still we find that between forty and fifty years of age there was a reduction of unvaccinated admissions to the extent of seven eighths, while among the vaccinated there was a reduction only to the extent of about four fifths. Now, when we review these differences, quinquennial after quinquennial, we gather that, from the ages of fifteen to twenty onwards to the ages of forty to fifty, there has continued a progressive loss of protective power; but that whereas during the early years of life the progression was an increasing one,

the rate of progression of the loss of protective power from puberty onwards was a lessening one. Still it did progress.

Dr. Gregory's table similarly shows the actual increase in both the vaccinated and unvaccinated admissions during the fourth and fifth quinquennials of age, and also the remarkable increase in the quinquennial fifteen to twenty years. It also shows the diminution of cases in both series after the age of twenty-five years.

Mr. Crossley's Table (Table 15 *a* in Appendix), making allowances, perhaps, for the difference in the Scotch and English constitution, agrees sufficiently in result with that of Mr. Marson.

The Wirtemberg Table does not correspond exactly with those derived from the experience of our own Smallpox Hospital. There is a very much larger proportion of cases shown during the first two quinquennials of life, but this may be due, not to the Wirtemberg vaccination being such as to permit of a very early return of receptivity, but to obstacles in the way of the admission of very young children into our Smallpox Hospital. But this table also shows the rapid increase of cases in the quinquennial ten to fifteen, and the further increase in the quinquennial fifteen to twenty. After this, in the Wirtemberg series, the cases of post-vaccinal smallpox began to decline. But, the total number of cases dealt with being much larger in Marson's tables, the latter are consequently more worthy of reliance.

The effect of vaccination in protecting against the reception of smallpox contagion during the early periods of life, and the gradual loss of the protective powers as years advance, is also seen on comparing together the ages chiefly attacked in epidemics in the same locality, but at periods when the extent to which the population was vaccinated was very different.

The following table is constructed from some data furnished by M. Marc d'Espine.*

	Ages attacked.						
	0—5 . 5—10.	10—20.	20—30.	30—40.	40—50.	50—60.	60 upwards.
1. The epidemic of Carouge, 1828 (84 cases), in which $2\frac{1}{2}$ per cent. were vaccinated (Herpin)	44 . 30 74	6	3	1			
2. The epidemic of Geneva, 1845 (137 cases), in which 72 per cent. were vaccinated (Marc d'Espine)	34	41	39	18	5		
3. The epidemic of Geneva, 1858-59 (249 cases), in which 84 per cent. were vaccinated (Marc d'Espine)	13 . 15 28	53	77	48	32	8	3

We see here that the more extensive the vaccination of the population was, the less was the number of cases of smallpox occurring at the earlier ages of life, and the later was the period to which the power of receiving the contagion was deferred.

We may judge also to some extent of the great liability of young children who are unvaccinated to receive the contagion of smallpox from the fact that, out of 2137 unvaccinated children discovered in the London schools by Drs. Seaton and Buchanan, no less a number than 1010 were scarred by smallpox.†

Before leaving this part of my subject I have something to

* 'Archives Générales,' 5me sér., t. xiii.

† Op. cit., p. 86.

say about *the fatality of smallpox at various ages in the vaccinated*. Although fatality may be taken as the index of severity of the disease when grouping attacks at all ages together, it must not be regarded in this light alone when we are considering the attacks at different periods of life. As with receptivity, so also with the fatality of smallpox both in the vaccinated and unvaccinated, the peculiarities of the system pertaining to the various periods of life exercise a remarkable influence. On referring to Table 14, showing the fatality of smallpox in the vaccinated and unvaccinated respectively at different ages, it is observable that in both series the fatality is high under five years of age,* lessening in each quinquenniad to fifteen years of age, and then steadily increasing to the more advanced periods. Whatever the age that is inquired into, however, the vaccinated are in a much better position as regards the chances of recovery than the unvaccinated. I can see in this Table little evidence of any loss of protective power of vaccination, as years advance, against the fatal result of smallpox. The increase in the fatality as years run on is not due to this cause, but almost entirely due to the peculiarities of age. Were it true that the progressive increase in the tendency to contract smallpox is associated with an equally progressive tendency to a fatal result, both being equally due to the loss of protective power of vaccination, we should expect to find the per-centage of fatality following the same rule of progression as the number of attacks in each quinquenniad of age. Instead of this we find that on the whole it follows in the vaccinated the same law of variation that it follows in the unvaccinated. The differences that exist are such as to show the existence and operation of a controlling power in the case of the vaccinated.

* Hebra states, as the result of twenty years' experience of the General Hospital at Vienna, the fatality of newly born and very young infants unvaccinated at nearly 100 per cent. 'Papers,' &c., p. 137.

Thus, starting from the per-centage of deaths in the two series under five years of age, we find, in both series, that it is less in the second quinquenniad than in the first, and that the reduction in fatality is to much about the same extent. But, comparing the third quinquenniad with the second, we find that, notwithstanding a vastly greater receptivity on the part of the vaccinated, a reduction of fatality happens to the extent of more than a half, while the reduction of fatality in the unvaccinated is only reduced below that in the preceding quinquenniad to a very trifling extent. The difference must be attributed to the different position in which the vaccinated and unvaccinated stand. In the fourth quinquenniad, again, a similar slight increase of fatality is observed in both series, the natural increase of receptivity not being complemented by a similar increase in fatality. A similar amount of increased fatality is observed again in the fifth quinquenniad. In the sixth quinquenniad the increase of fatality is, in this table, slightly greater in the vaccinated than in the unvaccinated column. Again the same thing is observed in the decenniad thirty to forty. In the decenniad forty to fifty the increase of fatality is about in the same proportion in the two columns. If, then, any loss of protective power against the fatality of the disease takes place, it is not observable until after twenty-five years of age, and then does not appear to be very remarkable.

It is on account of this influence exerted by the systemic peculiarities of age upon the receptivity and fatality of smallpox, both in the vaccinated and unvaccinated, that mortality tables are so very inferior as guides to the determination of the influence exerted by vaccination in protecting individuals at different periods of life. Thus it is, for example, that the following table, given by Mr. Simon, fails to represent the whole truth of the contrast between vaccinated and unvaccinated populations, when attacked by smallpox.

*Proportionate distribution by age of 1000 smallpox deaths in Geneva before the discovery of vaccination, and of the same number in England, London, and Paris respectively, at periods subsequent to its general practice.**

Ages.	Geneva, 1580—1760.	England, 1839 and 1847.	London, 1818—1851.	Paris, 1842—1851.
0—5	805	739 $\frac{1}{4}$	684	338
5—10	155 $\frac{3}{4}$	127 $\frac{1}{2}$	131	59
10—15	18 $\frac{1}{2}$	24 $\frac{1}{2}$	29 $\frac{1}{2}$	} 132 $\frac{3}{4}$
15—20	8	25 $\frac{1}{4}$	30	
20—25	5 $\frac{3}{4}$	30 $\frac{1}{4}$	48	} 329 $\frac{1}{2}$
25—30	4 $\frac{1}{2}$	18 $\frac{1}{2}$	35	
30—35	} 2 $\frac{1}{2}$	{ 11 $\frac{1}{2}$ 7 $\frac{3}{4}$ 15 $\frac{1}{4}$	19 $\frac{3}{4}$	} 109 $\frac{1}{2}$
35—40			12	
Over 40			10 $\frac{1}{2}$	31 $\frac{1}{2}$
Total.	1000	999 $\frac{3}{4}$	999 $\frac{3}{4}$	1000 $\frac{1}{4}$

But it is of interest as showing the displacement of the smallpox mortality which has taken place since the introduction of vaccination. Mr. Simon calls special attention to the column relating to Paris, as showing some remarkable instability in the results of French vaccination, and hints that the quality of the lymph currently used in France may be a cause of it. But this is a matter not now under discussion.

5. *Is there any reason to believe that, in the majority of vaccinated persons who exhibit a return of the capacity for developing in their system the virus of smallpox or its substitute the vaccine virus, the return of the capacity is gradual and due to a progressive weakening of the protecting power of the vaccine disease by lapse of time?* We may, in

* 'Papers,' &c., p. xxxi.

seeking for a reply to this question inquire—(1) Whether there are any facts which show that the capacity for developing the smallpox virus introduced by inoculation is greater as years advance from the time of vaccination; that is, whether inoculation succeeds more certainly as time advances. (2) Whether the smallpox that occurs in vaccinated persons is less modified in its character as years advance. (3) Whether revaccinations succeed more certainly when practised upon persons at later than when practised upon persons at earlier periods after their primary vaccination. And (4) Whether the results of the revaccination are more perfect and less modified as years advance.

(1) We have no sufficient number of observations to assist us in replying to the first of these inquiries. All we know is, that *inoculations* have failed, in the hands of many observers, at periods of from twenty to forty years and more after the vaccine disease has been undergone; but, on the other side, there are numerous observers who have found their inoculations succeed from ten to twenty years after a primary vaccination,* so that, tested in this way, it is certain that the capacity for developing smallpox virus, destroyed absolutely by vaccination, may return after an interval of very few years, and may not return after an interval of many years. Besides, we have already seen that receptivity of contagion may be recovered within a year of vaccination, and this involves return also of capacity for development of the virus.

(2) I may say, with respect to the severity with which casual smallpox affects persons at different periods after vaccination, that it does appear that the severity of the disease, when it occurs, is, on the whole, greater as years advance from the period when the vaccine disease was undergone; and, moreover, that the degree of severity at different ages

* Instances are cited by Steinbrenner, op. cit., p. 462.

seems to indicate that there is a gradual loss of protective power against the operation of the virus. Between the ages of ten and fifteen years there is, in addition, some other cause in operation which tends to render the development of the smallpox virus more perfect than at any other period of life. I have calculated, upon the numbers given in the Wirtemberg table (Table 16), the proportion borne by the cases of varioloid to all cases of the disease occurring in the seven quinquenniads from infancy to thirty-five years of age, and the result is that, starting with 85 per cent. of varioloid cases in the first quinquenniad, the proportion rises slightly in the second quinquenniad, falls considerably in the third, rises again considerably in the fourth, and then very gradually falls to the seventh. In the fourth, fifth, sixth, and seventh, however, the proportion is lower than that in the first quinquenniad. Thus—

Proportion per 100 of cases of varioloid	Ages 0—5 . 5—10 . 10—15 . 15—20 . 20—25 . 25—30 . 30—35						
	. 85	. 86·7	. 77·9	. 83·2	. 83·6	. 82·5	. 80·0

The danger, then, of vaccinated persons in the years of early puberty is a twofold one—first, they have naturally a greater tendency than at any other age to develop the virus ; and next, it is in these years that the protective power of the vaccination against the reception of the contagion undergoes its principal weakening. It is fortunate that, in association with these conditions, there is no tendency imparted by age to a high fatality. Another fortunate thing is that, by the time the two next quinquenniads of age are attained, when receptivity is high in the natural course of things, the resistance to the operation of the virus is naturally greater than it was between the ages of ten and fifteen years, and the fatality has not yet become very considerable.

(3.) Are complete failures in revaccination less frequent whe

the operation is performed at a greater than at a less period of time from the primary vaccination ; and do they indicate anything like a progressive return of capacity on the part of the system to develop the virus when introduced? Table 12 shows the results of 11,565 revaccinations performed in the Wirtemberg army, arranged according to age. Do we find that the failures were less numerous at the advanced than at the earlier ages? The failures at ages under twenty years, that is, in persons old enough to bear arms and yet under twenty, were 338·7 per 1000 ;* in persons over twenty and under thirty they were much more numerous, namely, 455·2 per 1000 ; and they became much less numerous again after thirty years of age, namely, 366·2 per 1000. Now, what does this variation mean? If we had no other facts by which to interpret it, the explanation would be difficult ; but when we call to mind that between the age of fifteen and twenty there happens naturally a remarkable increase of receptivity for smallpox, we can scarcely avoid seeing here also the indication of a similar increase naturally (and independently of vaccination) in the capacity for developing the virus. In the increase of failures between twenty and thirty years we see, by the light of similar facts, an indication of a natural reduction in the capacity for the development of the virus connected with the peculiarities of age, just as we find the receptivity for contagion naturally lower in the decenniad twenty to thirty than in the years just preceding twenty. But the diminution of the failures in revaccination in persons over thirty years of age can only be explained by assuming a greater capacity for developing the virus than existed in the

* Dr. Frosch, of Wittengau, in Bohemia, writes ('Papers,' &c., p. 126) that he revaccinated 200 of his patients (who had been vaccinated in the first month of infancy) when they had attained from the tenth to the fifteenth year. The only result he obtained was a dry reddish papule, disappearing from the sixth to the seventh day, and leaving merely a little blue mark upon the skin.

decenniad twenty to thirty. It is not concurrent with natural variation in receptivity, for during this period natural receptivity is lowered considerably ; it can, to my mind, only be accounted for by assuming a decided lowering of the protective influence of the vaccine, permitting the more ready development of the virus than could have taken place between the ages of twenty and thirty years. In Dr. Duncan Stewart's work* there is a table of revaccinations performed by Assistant-Surgeon Russel, of Amjere, upon eighty-seven natives (abridged in Table 18 of Appendix). He only succeeded five times, a remarkably small proportion of successes, which leads one to think that under some different conditions it would have been larger. But there is this to be observed—that the five successes were in persons aged twenty-one, thirty, thirty-two, and thirty-seven years. So far, it confirms the inference I have drawn from the Wirtemberg table ; but I am not going to lay much stress upon it, because the table embraces persons whose primary vaccination was not performed always in infancy, as may fairly be presumed to have been the case in Wirtemberg. Russel's five successes may be open to a very different sort of explanation.

(4.) When revaccination succeeds, is the success more complete as years advance from the primary vaccination? The Wirtemberg table again replies to this as respects persons at an age to bear arms, namely, from puberty onwards. And it shows us that the susceptibility of the system to the operation of the vaccine virus a second time becomes more and more perfect as age progresses. Under the age of twenty years the complete successes were only sixteen in excess of the modified successes ; at ages from twenty to thirty the complete successes out of a smaller proportion of successes of all kinds were twenty-six in advance of the modified successes ; and at ages above thirty years the complete successes

* *Op. cit.*, p. 165.

were actually more than twice as numerous as the modified successes. Now, what does this teach us? Why this, that the loss of protective power against the development of the virus over thirty years of age is associated still more remarkably with a return of high susceptibility to the operation of the virus, inasmuch as two thirds of the successes were complete. It teaches further that the *natural* increase of capacity for development of the virus at full puberty, a few years prior to the age of twenty, is not associated with an equally remarkable susceptibility to the full operation of the virus, for the latter is actually less than it is at the age of twenty to thirty, at which the capacity for development of the virus at all is not so great; and very much less than the susceptibility for the operation of the virus at ages above thirty, at which the capacity for development at all is very little less. There is also as much correspondence as could be expected between these results of re-vaccination and those given by the Wirtemberg table (Table 16), of the proportion of varioloid cases in persons attacked with post-vaccinal smallpox. In both tables the susceptibility to full and complete development of the virus is not very different in the periods fifteen to twenty and twenty to thirty, whereas in both a tendency is shown to the permission of a more complete development of the virus when thirty years of age are passed.

Let me now briefly sum up the conclusions I have arrived at on this part of my subject. The question is this—is the protection against smallpox afforded by vaccination limited in duration? My answer is—1st. That in the large majority of vaccinated persons the protection against casual smallpox, obtained by the time that the vaccine disease has completed its course, lasts for the whole remainder of life. Except in a small number of persons, vaccination is a life-long protection against all ordinary and even against extraordinary chances of contagion. 2nd. That in a considerable

number of persons the capability of developing the virus, when introduced into the system even, is not restored at any future period of life ; but that in the majority of vaccinated persons this condition of the system is recovered partially or completely, but that its recovery is of no practical importance unless there be a readiness to receive contagion at the same time. It only indicates that, on occasions of such exposure as may be sufficiently prolonged and intense as to overcome a tardy receptivity, certain persons are endangered. There is existent at all times a larger number of vaccinated persons whose system is in a condition to develop the virus, than of vaccinated persons who, except under very unusual exposure, are capable of admitting the contagion. 3rd. That among the small proportion of vaccinated persons who, in the course of their lives, reacquire a power of receiving the smallpox contagion into their system, this power arises from a progressive weakening of the protection originally conferred by vaccination ; that the loss of the protection is progressive ; that the rate of progression is greater from infancy to puberty than at any other period of life, and greatest of all in the period of early puberty, during the years just preceding the age of fifteen,* after which it becomes slower ; and lastly, that the weakening of the vaccine protection permits of the natural tendency of certain periods of life to the reception of smallpox contagion being distinctly brought into play. 4th. That the weakening of protective power as respects receptivity is not associated with any unusual tendency to fatality, so far as is observable, until the age of twenty-five years, and even then it is not enhanced by the loss of protection to any very remarkable degree. Whatever the distance of time from vac-

* This result derives some confirmation from the fact that it was not until somewhere about the year 1815 that cases of varioloid were observed often enough to convince the profession of the occasional return of receptivity for contagion.

ination, smallpox is a less fatal disease than among persons not protected by vaccination at all. The variations of fatality of post-vaccinal smallpox are almost entirely due to the constitutional peculiarities attached to the age of the persons invaded by the disease. 5th. That the return of the capacity of the system for the development of the smallpox virus (or its substitute) is due to a progressive weakening of the protection conferred by the revolution effected in the system by the vaccine disease; and that this recovery of capacity for development of the virus is most remarkable (in persons over fifteen years of age) after the age of thirty years is attained; and lastly, that the progressive weakening of the protection permits of being brought into play a natural tendency to the development of the virus due to constitutional peculiarities belonging to the age of full puberty (fifteen to twenty years). 6th. Finally, that, as years advance after vaccination, the progressive weakening of the vaccine protection is also shown by the more complete manner in which the virus of smallpox (or its substitute) is able to evolve itself; that in this respect the weakening of protection is not very remarkable until after the age of thirty years has been attained, up to which age the vaccine disease very slowly loses its power of modifying the operation of the virus; and lastly, that the weakening of the vaccine protection in this respect permits of being brought into play a natural tendency to more complete development, due to constitutional peculiarities, attaching to the age of ten to fifteen years, at which also there happens the most rapid loss of vaccine protection against casual contagion.

Having established these points, we may proceed to inquire into the *causes* of the loss of protection which occurs as years advance from the period of vaccination, and how far the operation of these causes is capable of being prevented or controlled by man, or the results of their operation remedied.

To what causes, inherent or otherwise, can the occasional return of receptivity for the contagion of smallpox, and the frequent return of the capacity for developing the virus in the system, be referred? How far is it in our power to obviate them, to control their operation, or to counteract their results?

There is to be observed throughout organised nature the operation of a law the tendency of which is to remedy disturbances and to restore imperfections. We see it in operation in the vegetable world when we examine the monster which we call a "double flower." We see here the metamorphosed leaf, which under ordinary circumstances forms the stamen, widened out, veined, and coloured—the first step towards its primitive and normal condition. In other instances even the constituent elements of the pistil unfold themselves, and acquire something of the colour and shape of the ordinary leaf; and in some instances the whole of the constituent elements of a flower-bud assume more or less of their primitive foliaceous aspect and colour, manifesting thus the nature of the organ which forms their primitive or normal type. In other plants we may see the shortened internode which gives rise to the "opposite" position of leaves, lengthening out to such an extent that the attachment of the leaves is no longer precisely opposite, but such as to enable us to trace somewhat of the spiral arrangement upon the stem which botanists tell us is the typical arrangement of all foliaceous appendages. In the animal world we see it in operation most distinctly in the repair of all kinds of accidental injuries, as well as in the tendency to a return to health when any cause has been in operation effectually to disturb the normal performance of functions. We see it sometimes in the complete restoration of lost parts, sometimes in the

reparation of injured parts, the consolidation of a fractured bone, the healing of a flesh wound, and the formation and gradual assimilation of a cicatrix to the structure and aspect of the surrounding skin. To use the expressive language of Mr. Paget,* “it is a law wider than the grasp of science—the *law* that expresses our Creator’s will for the recovery of all lost perfection.” It can, therefore, be no matter of surprise to any one accustomed to take broad views of scientific subjects to find that the normal condition of the system by virtue of which it is capable of receiving and developing a morbid virus should exhibit a disposition to recover itself when disturbed or destroyed by its previous operation. Such recovery, or a *nisus* to such recovery, is rather a thing which he would anticipate; it would be nothing more than an instance of that “striving after lost perfection” which he is accustomed to observe, more or less effectual, in whatever direction he casts his eye. And, as a fact, such recovery does take place after the operation of other viruses than that of smallpox or of vaccine has been completed. Scarlatina, measles, hooping-cough, &c., are maladies which, when once passed, leave the system for a long period, and mostly for the whole remainder of life, incapable of receiving a second infection or of again developing the disease; and yet a repetition of each of these maladies is occasionally met with, of some more commonly than of others. Nor could such a person wonder at all that the recovery of the normal condition of the system is a gradual and progressive affair. It is many years before the scar of a wound comes at all to resemble, even in colour, the skin which surrounds it; longer still before its structure is thoroughly assimilated to it; and longer yet before it becomes altogether undiscernible. The wound must be very slight for this to happen before many years have passed. So, too, he cannot wonder—he would

* ‘Lectures on Surgical Pathology,’ vol. i, p. 166.

rather expect to find—that the recovery of susceptibility to any disease, once the susceptibility is destroyed, would be a slow and gradual process, and that it would pass through many stages of incompleteness before its perfect restoration was attained.

To apply this law, thus general in its extent, to the case before us, it can be nothing to occasion surprise that now and then we meet with persons in whom a power of receiving smallpox contagion, once lost with the occurrence of an attack of that malady, is after a lapse of years recovered; that now and then such persons are met with who are capable of developing the virus after the power of development has been destroyed by its previous operation; that, in the earlier stages of restoration, a result of its operation would follow less complete in its character than at later stages of restoration. Nor would it cause surprise to discover that it is only in a small proportion of cases that a recovery takes place at all. And so also with vaccinia. This is not, like smallpox, a disease natural to man; it is less readily communicated, therefore, to him, and the impression made upon the system when it is communicated is less profound and searching. It is only in very rare cases that the development of the vaccinal fever is like that of the variolous fever associated with general eruptive phenomena. The injury which it inflicts, therefore, upon the natural constitution of man is less deep than that inflicted by an attack of smallpox. We should expect that it would be so; and as the scar of a slighter wound is restored to the natural appearance of skin sooner than that of a severe wound, so we should look naturally for a more ready recovery of the perfection of the constitution after it had been lost from the operation of vaccinia than if it had been lost from the operation of smallpox. Now, as regards receptivity, we have seen that cases of post-vaccinal smallpox are much more frequently met with than cases of post-variolous smallpox; that,

as regards contagion, it is more readily received by vaccinated than by variolated subjects; and, as regards the return of capacity for redeveloping the virus in the system, we find from the Wirtemberg table (Table 12) that the successes of adult vaccination were more frequent, on the whole, in those who had been previously vaccinated than in those who had undergone an attack of smallpox. I am comparing here the post-variolous vaccinations and the revaccinations of persons with complete scars upon their arms.

I have said thus much upon this subject because, in discussing the causes which may be believed to reduce the chances of permanent protection against smallpox after vaccination, it is needful to keep well in mind that, in all persons who have undergone smallpox, vaccinia, or any other disease which disables the system from its reproduction, there nevertheless exists a natural tendency to a return to the pristine condition, more marked and stronger, as we shall see in the sequel, in some individuals than in others—naturally stronger. And when we speak, as I am about to do presently, of the *causes* of return of receptivity or capacity for development of the virus of smallpox after vaccination, we are not using the word in the sense of active causes, but in the sense of *permissive causes*, causes which operate as such because the conditions which constitute them are such as fail more or less to oppose a sufficient resistance to the operation of the law of natural restoration of lost perfection.

Now, I think, after all that has preceded, it must be evident that, in a large majority of instances in which a return of receptivity for the virus of smallpox takes place, the permissive cause in operation is some fault or defect in the primary vaccination, or rather in the results of the operation; some fault in the perfection of the vaccine disease as an exanthematous malady, and thus in its completeness as a

substitute for smallpox. It will be recollected that I have established the fact that the chances of escaping the *reception* of the virus on exposure to contagion are lessened remarkably by the production of multiple vaccine vesicles, the protection against contagion being nearly four times greater in those with three or more scars upon their arms than in those persons with only one vaccine scar upon the arm. It also appears from the facts in our possession that the character and perfection of the vesicles (putting their number out of the question) is a matter of no importance. It is difficult to account for this ; but the fact still remains. Then, as respects the *capacity for developing* the virus introduced, we have seen that it is less likely to return, and less likely to return in such a manner as to permit the complete development of the disease, in those with multiple vesicles than in those with single vesicles, and less in those with good vesicles than in those with small and imperfect vesicles. In fact, the return of capacity for developing the virus is very distinctly and unmistakably associated with an imperfect development of the vaccine disease, and an imperfect and, as it were, superficial alteration effected in the blood.

We have now, therefore, to inquire whether experience has taught us anything as to the conditions which render the operation⁴ of vaccination so little successful as to leave the system open thus to the reception and development, at some future time, of the smallpox virus. The conditions which influence protective success are various, and have reference to the constitutional and other individual peculiarities of the person vaccinated, to the circumstances under which the operation is performed, to the method of vaccination adopted and the carefulness of the operator, to the manner in which the pock passes through its several stages, and to the source from which the virus used is derived.

1. *The influence of individual peculiarities.*

The first of these peculiarities that I shall consider is *race*. It is an opinion which has found its way into text-books that the dark races of mankind are less susceptible to the protective influence of vaccination than Europeans, or rather, I should say, that the operation itself is more apt to fail of success. It is on this ground that Dr. Copland and others express an opinion that inoculation of smallpox virus is a measure more adapted to their peculiarities than the protection introduced by Dr. Jenner. I cannot help thinking that the evidence upon this score is wanting in precision, and that the inquiry is open to fallacies which have not been sufficiently allowed for. I have already noticed the observation that in the natives of India the vaccine vesicle does not usually acquire the full development that it does upon the skin of the European vaccinated from the same source and at the same time, notwithstanding that the lymph employed in operating upon the latter may be taken from the smaller vesicle of a native child.* But yet there is proof that, small and comparatively incomplete as the vesicles are in the native, the vaccine disease is undergone by them in a manner such as to impart protection against the operation of the virus of smallpox introduced by inoculation; while the revaccinations performed by Assistant-Surgeon Russel exhibited an amount of failures by no means inferior to those observed in European practice. Besides which, we are assured by Indian observers that, so long as the vaccine disease is undergone in a normal manner, the protection which it affords to the native against contagion is equal in all respects to that conferred upon the European.† A recent observation made

* Smith of Mysore. Duncan Stewart, *op. cit.*, p. 159.

† Dr. Cameron, in his Report to the Medical Board of Bengal on the Epidemic of 1829-30, says, "There does not appear to be one authenticated case of variola supervening on perfect vaccination, provided the process has

by Dr. Moriarty,* to the effect that he has rarely seen vaccination take with the African or West Indian adult, is not to be accepted as any evidence of insusceptibility to its influence. It is difficult to conceive that [a constitution so ready to develop the virus of smallpox as that of the African is, should be incapable altogether of responding to that of vaccine. Vaccination may fail in tropical countries from other causes besides the race of the person who is the subject of the operation, and we should require to know, not only whence the lymph used in it was derived, but also whether it equally failed at the same time upon European subjects. So far as appears, protective vaccination was carried out by Drs. Bowerbank and Turner without any difficulty at the time when smallpox visited the island of Jamaica in 1851, and Dr. Miller† tells us that, with lymph obtained from about half a grain of a vaccine scab multiplied by successive transmissions, he managed in the course of two months to get through 3500 vaccinations among all classes of the community. It is upon loose statements such as that I have alluded to, repeated by one person after another, that current opinions upon such subjects are not uncommonly found to be based.

In respect to individual *temperament* I need say little. Common observation has established the opinion among vaccinators that in infants with fair skins and light hair the vaccine eruption develops itself more fully and finely than it does upon children who are of a darker complexion. I do not know, I have never heard, that the latter obtain from

been carefully and perfectly conducted on a healthy subject. I think, under these circumstances, we may conclude that vaccination is as good an antidote against smallpox in Bengal as the same process, *ceteris paribus*, performed in any other country where this valuable discovery is known.” Duncan Stewart, *op. cit.*, p. 144.

* ‘Medical Times and Gazette,’ 1866, vol. ii, p. 664.

† ‘Med. Times and Gaz.,’ 1867, vol. i, p. 441.

vaccination, however, a less amount of protection than the former.

The influence of the *age* at which vaccination is performed will require more discussion, inasmuch as it involves the question as to what is the proper age for the vaccination of infants. Now, although newly born infants are, perhaps, less receptive of smallpox contagion than they become after the lapse of two or three months, it is still possible that they may admit the virus into their system at the earliest ages, and hence it is a matter of importance to know whether vaccination performed upon them is equally perfect in result and equally protective as when performed at a later period of life. It is quite possible that receptivity of contagion may be very small in young infants, and that it is difficult for them to catch smallpox except in epidemic seasons, and under more intense and prolonged exposure than ordinary, and yet that the capacity for developing the virus may be present in all its force. That it is so in some is proved by their being born into the world with smallpox upon them or by the eruption appearing within the first few days of birth.

First, then, as to the development of the virus when vaccination is performed very early after birth, a practice which, in the presence of smallpox in a family, has been adopted even a few hours after the child has come into the world. Husson stated in 1821 that, after twenty years' experience, during which he had vaccinated at all ages, from immediately after birth to extreme old age, he had never been able to observe any remarkable difference in the results of the operation. It is also stated in the French report upon vaccination in 1860* that, during six years, it had been customary to vaccinate every week several newly born children from the Clinique d'Accouchement of the faculty, and

* P. 18.

that the reporters had constantly observed that everything proceeded in such infants just as it did in children more advanced in age. They deny that very young infants are less liable to take smallpox than after they are a little older, attributing their apparent immunity to the peculiar circumstances in which they are placed, which very greatly lessen the chances of contagion. They state that, during the year 1860, they had themselves seen smallpox happen in two infants seventeen days old, and in a third eighteen days old, at the Hospice des Enfants Assistés. But still it does sometimes happen that the capacity for developing the virus is very small in young infants, and hence either the vaccination fails altogether or is productive only of a modified result. Should it thus fail, however, there remains open the resource of making a second attempt at a later period. If vaccine virus cannot be developed, it is not very likely that there is a sufficient receptivity for smallpox contagion to take effect. Steinbrenner* cites among others the following instances :—An infant a day and a half old, whose brother had smallpox, was vaccinated without success, and failed to catch the disease also. An unvaccinated infant three weeks old was shut up with a patient who had varioloid without taking smallpox; this child was also vaccinated without success. An infant five weeks old who remained constantly with a woman ill with smallpox failed to take it; this child was vaccinated four times without result. An infant ten weeks old, although shut up in the same house with a sister who had smallpox, remained unaffected; this child was vaccinated twice without success. The following case, although open to some criticism, may be cited as probably showing that the possibility of developing the virus by an infant does not necessarily involve receptivity of contagion. It is that of an infant five weeks old, who remained with his mother for three days after a varioloid eruption had appeared

* Op. cit., p. 471.

upon her. He failed to take the disease, but was nevertheless vaccinated with success. There can also be no doubt that not only may the power of developing the virus both of smallpox and vaccinia be absent in young infants altogether, but that it may be so imperfect in other instances as not to permit of the complete manifestation of the disease upon the skin, although the reception of the virus may take place. Thus, Heim observed thirty-four cases of unvaccinated children in the first period of their life, who, under the influence of the epidemic contagion of smallpox, admitted the virus and developed the disease, but still in whom it only took the varioloid form, and Steinbrenner records* a number of instances where infant vaccination produced only modified results. Another fact also appears as the result of observation, namely, that an infant may be altogether insusceptible of developing the vaccine virus, and yet become capable of it at a later period of life, although at first, perhaps, incompletely. Steinbrenner vaccinated a child four months old on three successive Tuesdays without obtaining any result. He vaccinated him for the fourth time when eleven months old, and now did obtain a result, but it was a modified one. In many other instances he obtained at first only a modified result or no result at all, but on repeating the vaccination from one to three years later good and normal pustules were obtained.

It is not improbable that sometimes a resistance on the part of the system of an infant to the operation of the vaccine virus may be imparted *in utero* by the fact of the mother having undergone an attack of smallpox during her pregnancy. Two cases are recorded by Dr. A. Hall,† in which vaccination of the infant failed under such circumstances,

* Op. cit., p. 474.

† 'Yéar-Book' for 1861, New Sydenham Society, p. 391.

although no pitting of the children's skin was observed. Dr. David relates a similar case. I have myself met with an analogous immunity against cattle plague contagion conferred upon a calf, the mother of which suffered an attack of that disease when she was five months gone in her pregnancy. On two occasions this calf was very freely exposed to the contagion of the disease without taking it, while other calves similarly exposed at the same time suffered in consequence of the exposure.

It appears thus that, although the majority of infants are capable of a full and satisfactory development of the vaccine virus, there are some who are incapable of developing it at all, and others who develop it imperfectly ; and further, that the capacity for development, which may be imperfect at first, may become more complete as infancy advances into early childhood. Some practical inferences arise from this. One of them is that a failure in vaccination in infancy should not be finally accepted as evidence of incapacity to develop the virus, or as showing anything more than a temporary immunity from smallpox ; and hence that, from time to time, renewed attempts should be made, at intervals of several months, to obtain a successful vaccination. Another is that a modified result in infancy should not be regarded as satisfactory, but that at a later period a second attempt should be made, as with those who refuse to take the vaccine altogether. A third inference is that, although as a protection against smallpox vaccination should be performed in infancy, and although when smallpox is in a house or family it is the duty of a medical man sometimes to vaccinate an infant immediately after birth or within a few days of birth, it is somewhat questionable whether *very* early infancy is, under all circumstances, the best time to select for the operation. It appears doubtful whether the full capacity for developing the vaccine protectively is acquired so early, and whether infants vaccinated

very early are not likely to gather again more surely and quickly than others a renewed capacity for its development, and with it a readiness to receive contagion. One thing appears certain, that at or just before the period of puberty persons vaccinated in infancy, in a certain proportion of instances, do acquire afresh a remarkable readiness to take smallpox, and this readiness is, as puberty advances into early manhood, assisted by the natural changes which the constitution undergoes at that time. We have nothing to guide us in distinguishing between those whose primary vaccination is sufficient to protect and those whose primary vaccination has ceased to protect except the appearances upon the arm. Where the primary vaccination has, from constitutional incapacity or from any other cause, resulted in one or two poor vesicles and abnormal scars, we may be certain that a renewed protection should be sought, not because such persons are certain to be insufficiently protected, but because it is among such as these that smallpox is most likely to select its victims. In cities and closely packed communities no considerations of duration of protection should deter us from infant vaccination; but in families living apart and at a distance from others, and having little or no communication with them, I cannot avoid thinking that an advantage might be gained were vaccination deferred to a rather later period than that at which it is usually practised.

One of the personal causes which is apt to render the development of a primary vaccination unsatisfactory is a *state of ill health at the time the operation is performed*. It is not necessary that actual manifest or localised disease should be present to interfere with success. A child who is cachectic, from any cause, is likely to develop the vaccine disease imperfectly, and should such a child take smallpox just before or at the time of vaccination or subsequently, it is likely, nevertheless, to have a fatal result. Infants who

are puny and badly developed at birth are thus apt to furnish bad successes, and, as they grow a little older, it is desirable to renew their vaccination. And it is the same with badly fed and neglected children. But to proceed to positive diseases and constitutional disturbances. Jenner noticed that the vaccine disease was modified in its progress by certain diseases of the skin; so also did Willan and several others of the early vaccinators, whose experience has since received a very general confirmation. Still, there are many cases constantly happening which show that this is not a necessary modification. Cutaneous eruptions are apt to accompany the disturbance of the constitution which dentition gives rise to; and the process of dentition itself, by giving rise to ill-health, may of itself disturb the evolution of the vaccine virus. Hodenpyl believed that preoccupation of the system by rickets or scrofula, or even, by scabies, interfered with the success of vaccination; and Cross quotes Elässer to the effect* that, in a certain district under his cognizance, where scabies was very prevalent and numerous cases of modified and regular smallpox occurred in vaccinated children, he had discovered, on inquiry, that the insufficient protection had been given by the lymph taken from a child with scabies having produced only an irregular and modified vaccine disease. In my own examination of school children I met on several occasions with children with thickened and ichthyotic epidermis; and whenever this happened I invariably found the scars of vaccination small, flat, and unsatisfactory. Observers differ also as to the influence of more severe acute diseases upon the course of vaccinia. Krauss (quoted by Steinbrenner†) observed that, when the acute exanthemata, such as scarlatina, measles, chicken-pox, or miliaria, appeared during the first days after vaccination, the rising of the vaccine

* Cross, *op. cit.*, p. 196.

† *Op. cit.*, p. 743.

vesicles was deferred even as late as the tenth or fourteenth day. But when they did not appear until the sixth, seventh, or eighth day, the vaccine vesicles, being little developed, remained stationary, and did not continue their progress until the exanthematous malady subsided. Other observers, however, have failed to observe any such phenomena, and altogether we must infer that they are not constant accompaniments of this complication. The Wirtemberg observers have found the vaccine disease modified, and the vaccination even rendered wholly unsuccessful, by the presence of catarrhal fever, epidemic diarrhœa, and whooping-cough; and Steinbrenner relates that he failed, although he made eight punctures, to produce vaccinia in a child who, during an epidemic season, was attacked with influenza; yet a few weeks later, and after recovery, he succeeded perfectly.*

A case is related by the same writer where an attack of pleurisy caused the vaccine eruption to dry up on the eighth day; vaccinating again, after recovery from the attack, he obtained a complete success. In another case he mentions, an attack of pleurisy retarded the appearance of the vaccine eruption for as long a period as five weeks, but at the expiration of this period it passed through its regular course.

The use of *active medicines*, especially purgative medicines and mercurial purges, has an undoubted influence in checking the development of vaccinia. Lüders, Buchholz, and others, give their testimony to this influence; and an intelligent and active vaccinator in my neighbourhood, who had never heard of this influence, told me of his experience in

* A parallel observation was made by Hufeland, who, in April, 1788, inoculated fifty subjects with smallpox virus, and succeeded in every instance. From the commencement of May, however, he was frequently unsuccessful, and he attributed his want of success to a prevalent epidemic catarrh, which hindered the development of the virus. He obtained a primary and local result, but as soon as the catarrhal fever began the general eruption which was commencing over the body became arrested.

respect to it as something which he thought new. He says he has often prevented the taking of the vaccine by giving the infant a calomel powder, and caused the vesicles to wither prematurely by the same sort of dose after their appearance. Now, it is well that this fact should be extensively known, because it is, I believe, a very common practice among vaccinators in some districts to give a dose of this kind on the child being brought on the eighth day for inspection. The mothers ask for it, and it is given as a matter of course. Jenner believed that he had seen the administration of sulphur similarly prejudicial.

But apart from all these causes calculated to interfere with the development of the vaccine virus, *there may exist in an individual naturally an imperfect capacity for developing the virus*, existent at infancy, and not improving during the rest of life. It is only in this way that I can account for the curious results of some of Heim's revaccinations, and also for the observation of Mr. Marson to the effect that often, when a primary vaccination takes badly, it will never afterwards take properly. Mr. Marson adds, "yet the individual may take smallpox severely."* He does not state on what special observations, made upon children thus unsuccessfully revaccinated, he bases this alarming opinion. *Primâ facie*, I should be disposed to question its accuracy, although children generally having bad marks on their arms must be admitted to be liable to fatal smallpox. It may be, and on the view I have adopted it probably is the truth, that, where a bad scar results from defective power of development in infancy, and revaccination shows that the capacity for complete development is not restored in after years, such individuals would not be so liable to take and suffer severely from smallpox as those whose bad scars arise from other causes of a more temporary character. The results of

* Loc. cit., p. 386.

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Heim's Wirtemberg revaccinations that I have referred to are such as come out on calculating the per-centages of success, complete or modified, and of failures, in cases where there were bad scars of primary vaccination upon the arm. On referring to Table 11, and again to Table 12, it is to be remarked that the failures of revaccination were rather more numerous when the cicatrices of primary vaccination were bad than when they were good. In the army vaccinations for 1836 we find the failures in those persons who had bad scars formed 44·2 per cent.; while the failures in those who had good scars formed 42·0 per cent. The five years' experience given in Table 12 furnishes a similar result. In those with bad scars the failures were 46·04 per cent., and in those with good scars only 40·92 per cent. The difference in the failures was very remarkable in those with one and two scars only. Thus, in the persons with *one* bad scar the failures in revaccination were 41 per cent.; while in those with one good scar they were only 33·7 per cent.; in those with *two* bad scars they were 49 per cent., while in those with two good scars they were 43·9 per cent. Again, on comparing the complete successes in those with good and bad scars respectively, we find that they were more rare with those having bad scars than with those having good scars. Thus, when there was *one* bad scar, a complete success of revaccination happened in 28·4 per cent., while with one good scar in 34·4 per cent.; with *two* bad scars a complete success was attained in 25·9 per cent., while with two good scars it was attained in 26·9 per cent.; with *three* bad scars in 25·1 per cent., and with three good scars in 26·3 per cent.; with *four* bad scars in 27·8 per cent., in those with four good scars in 28·4 per cent.; in those with *five* bad scars in 26·8 per cent., and in those with five good scars in 28 per cent. Above this number the primary vaccination may be regarded as exceptional. Now I see no way of accounting

for this result except by assuming that the same cause which assisted in rendering the primary vaccination imperfect, where the scars were bad, was still in operation in a certain number of the same subjects when the revaccinations were attempted, and that this cause was a constitutional inaptitude for the proper development of the virus.

2. The circumstances under which the operation has been performed.

The influence of *atmospheric temperature* upon the success and course of vaccination has been noticed by many observers. Thus, in a report of the Central Committee of the French Academy for 1821-22, it is stated that M. Ives,* a physician at Montluçon, vaccinated in the month of August twelve infants in the middle of the day, when the heat was stifling, and not one of them developed the disease; in the evening, when it was cooler, however, he vaccinated twelve more, and, although he used lymph from the same source, the vaccination was followed by complete success. Bousquet quotes the experience of M. Catel of the Île St. Louis in Senegal, to the effect that, at the time when the heat was very great, he has been compelled to vaccinate twenty or thirty subjects in order to get two or three vesicles, so that at such times he has on several occasions lost his supply.

Season and *climate* also appear to exercise a powerful influence. Sometimes their operation may be referred, as in M. Catel's instance, distinctly to the effect of temperature, at others this is less distinctly traced. M. Bousquet notes† how, at certain seasons, his vaccinations were less successful than at others. He particularly mentions that this was the case in the month of May, 1830, and that others met with a similar difficulty at the same time as himself. A similar

* Quoted by Bousquet, *op. cit.*, p. 31.

† *Op. cit.*, p. 28.

event was reported to the French Academy by M. Gouville of Carentan, who states that in April and May, 1832, his vaccinations succeeded perfectly, but that in June he began to meet with unusual failure, and that in July and August he failed so often, although vaccinating from arm to arm, that he had to repeat the operation two or three times before it would take. The remarkable series of vaccinations at the General Hospital of Nantes, which I have previously mentioned, appear also to illustrate the influence of some seasonal condition which it is not easy to comprehend. M. Bousquet attributes the anomalous results on this occasion to atmospheric causes, such as caused his own failures in 1830. At certain seasons of the year the climate of India appears to be very unfavorable to the development of vaccinia. Dr. Duncan Stewart's work abounds in illustrations of this, gathered from reports sent in by the district vaccinators. Dr. Stewart states that the difficulty in Bengal occurred in almost every rainy season, and was perceived from the month of May to that of September. Indeed, in the year 1837, it was feared that the supply of lymph would actually be lost. He thus describes the effect of the season*—"The vesicles at this time became extremely minute, the surrounding induration small, the areola diffused and ill defined, and the course of the disease hurried and unsatisfactory. Yet, strange to say, notwithstanding these unfavorable appearances, I have observed them uniformly to disappear on the approach of cool weather, and the disease resume speedily in the month of November the perfectly developed Jennerian character." In Bombay, Dr. McLennan† states the deterioration has invariably occurred in the hot months of April, May, and June, but that during the rains and in the cold weather the disease was as perfect as we ever see it. In the hot weather the proportion of failures was very great, and on one or two

* Op. cit., p. 152.

† Ib., p. 157.

occasions he also saw reason to apprehend the loss of his supply of lymph. In 1841, the superintending surgeon of Bombay wrote that at Mahableshwur the climate was such that, during four months of the year, it was impracticable to carry on vaccination; and Mr. Lamb of Dacca wrote that he had observed at Sylhet and Chittagong, and wherever the rains were heavy, that the virus was apt to degenerate in the rainy months, the lymph to become thick and muddy, and sometimes that the pustules degenerated into purulent ulcers. Macpherson also notices the bad development of vaccinia in the rainy season.* Dr. French observed a marked difference in smallpox when it attacked those who had been vaccinated in India, and those who in their youth had been vaccinated in England. In the latter the general character of the disease was that of modified smallpox, whereas all the fatal cases were found among those who had been born and vaccinated in India. This observation was made in the epidemic season of 1830.† It is interesting to observe how this variation in the development of vaccine corresponds with the season during which smallpox increases and declines in India. Dr. Morehead‡ describes the winter and spring as the seasons of epidemic prevalence of smallpox in India (January to July), the maximum of cases happening in March and April. Sydenham represents the vernal equinox in this country as giving the maximum of smallpox. Dr. D. Stewart also, speaking of the epidemic of smallpox in 1843-4, says that a few fatal cases occurred in the autumn; the first cool day in November added great activity, and the disease progressed in arithmetical ratio through the winter and spring; unchecked by the returning heat of March and April, it reached its height at this time, but yielded on the occurrence of the

* 'Med. Times and Gazette,' 1865, vol. i, p. 46.

† Duncan Stewart, *op. cit.*, p. 143.

‡ 'Diseases of India,' vol. i, p. 307.

early rains in May. Other observers elsewhere, in England and America, have observed the greater readiness with which vaccination succeeds in seasons where smallpox is epidemic; probably the same seasonal conditions are favorable to both. During the epidemic season of 1863 I was in constant communication with a number of vaccinators and medical men engaged in the same work, and they told me that they met with unusual success in the revaccinations which they performed at that time. In European countries it is commonly observed that the warm weather of summer tends to hasten, and the cold weather in winter somewhat to retard the progress of the vaccine vesicle.

3. The method of vaccination adopted and the carefulness of the operator.

I do not attribute very much of the success or non-success of vaccination to the particular mode which the operator adopts of introducing the virus into the system. It is of more importance that, whatever method be adopted, the operation should be conducted in a careful manner. But still I have a leaning towards the method of proceeding by scratches rather than by punctures, because I think I have observed that in the hands of ordinary men the failures are less frequent than when punctures are used.* The scratches are, perhaps, most readily performed by means of "the rake." When this instrument is used, and the virus smeared over the surface of about half an inch square, or rather less, a confluent group of vesicles results, upon which five to seven or eight may be counted, and a correspondingly extensive scar follows, which, by its size and appearance, may be distinguished from that left by a vesicle the product of a single puncture. Two such vaccinations, one upon each arm, will give all the

* Mr. Marson says that a good vaccinator should not fail of success more than once in 150 times.

protection that can be desired, if nothing occurs to prevent full development. Drs. Seaton and Buchanan* appear to be equally satisfied with myself as to the excellence of this method of vaccination (*viz.*, abrasion). "Some of the finest cicatrices," they say, "and the completest local results of vaccination that we have ever witnessed have been in children vaccinated in this manner. On the other hand, the scars resulting from vaccination by abrasion in the practice of some vaccinators were not above the ordinary size of puncture cicatrices; and we saw many children who were said to have been vaccinated in this way who were insufficiently protected. But, on the whole, we were satisfied that those operators who vaccinated by abrasion, scarification, or other similar method, produced the requisite amount of local effect much more generally than those who operated by simple puncture; the cicatrices produced by them being as a rule of better quality, both in respect of size and of the depression and characteristic pitting that indicate deep affection of the skin." There is certainly a very great difference in the results produced by different vaccinators, depending not only upon their method of introducing the virus, but upon the carefulness they exercise in selecting the vaccifer.† In my own school examinations of children I have often had no difficulty in naming the person whose hand has performed the operation in particular districts. The mark of the individual has been almost as easily read as his signature.

Among the principal causes of bad and unprotective vaccination, the fault of the operator, I may particularly allude to the use of lymph at too late a period in the course of the vesicle,‡ taking lymph from vesicles themselves badly

* 'Sixth Report of Med. Officer of the Privy Council,' p. 101.

† This term, introduced a few years ago by some French observers, is so convenient that I shall use it when speaking, as I shall have to do frequently soon, of the subject who furnishes the lymph.

‡ Drs. Seaton and Buchanan state, "out of 175 vaccinators, we met with

developed or imperfect in character, the use of dry lymph instead of arm-to-arm vaccination, or of the scab instead of the limpid lymph. The lymph for vaccination should never be taken after the areola is formed; so that, with the current lymph in use, the eighth day is the latest period at which it should be employed. When the areola is fully formed the lymph ceases to be limpid, and is imperfect in power and apt to give rise to imperfect or modified pustules. According to Sacco and Eichhorn's observations the secretion of the eruption is distinguished for energy on the fourth or fifth day, at a time so early that the nascent vesicle is not yet distinctly elevated above the general surface. A common fault I have seen committed is the taking of lymph in excess from some one vesicle. Steinbrenner believes that the lymph taken from an exhausted vesicle is imperfect in power. Dry lymph, under any circumstances, is less certain of operation, and the virus weaker than when used fresh from arm to arm. Common every-day observation attests this.* Unless under very exceptional circumstances, the use of the scab cannot be defended; but it has been found in hot countries that it is occasionally the only mode in which lymph can be transmitted from place to place, and by careful manipulation has sometimes answered its purpose very well; and so sometimes has

11 who took their lymph sometimes, and with 3 who took it habitually, at a later period than the instructions warrant. One of these considered that the vesicle was not mature until there was a large areola round it. All those who took lymph at a late period were house-to-house vaccinators."

* Drs. Seaton and Buchanan say (*loc. cit.*, p. 101),—"We often met with unvaccinated children in the schools, who said they had been 'tried' once, twice, three times without result, and invariably the operation (whenever we had the opportunity of ascertaining the point) was found to have been attempted with preserved lymph, generally by 'putting in the bones,' as the children called the use of points. Those vaccinators who used preserved lymph by rubbing it into an abraded surface had the fewest failures, but even these never attained the degree of success that attends well-performed arm-to-arm vaccination."

dried lymph, when not kept too long, and when not exposed to light and air. When the vesicles on the vaccinifer are small and poor they are apt to reproduce their kind, and in this way a supply of lymph may degenerate in quality. A careless vaccinator, who does not trouble himself to select the best pocks, and who does not vaccinate sufficiently extensively to procure a choice of pocks," may thus by his own negligence deteriorate his supply. This was an observation of Hufeland, and it corresponds with what has been noticed to occur in inoculations with the matter of a varioloid pock, which in the hands of Guillon, Sacco, and others, has preserved its modification, although transmitted through several generations. Still an observation of Bousquet's would seem to show that this deterioration of vaccine virus, if the virus itself be good to start with, does not occur permanently unless where repeated inoculations with from badly-selected pocks are made; in fact, that the virus may recover itself in a subsequent human generation. I refer to the inoculation of Passy lymph from a poor vesicle of the second generation, which produced an admirable result in the third. Cross* narrates an incident which illustrates remarkably the immediate evil result which may follow the use of lymph taken at a late period of the pock. Above 100 children, who had been vaccinated, had smallpox in Silesia in 1816, and some of them died. Dr. Kausch undertook an inquiry into the cause of the lack of protection, and discovered that they had all been vaccinated by the same surgeon, who, it was ascertained, had been in the habit of taking lymph as late as the eleventh day, often from vesicles that had been injured by rubbing or scratching, and at times had even raised an incomplete scab to obtain any moisture that he could find beneath it in order to vaccinate with it. The importance of selecting lymph before it has undergone purulent changes is confirmed by the

* Op. cit., p. 196.

analogy of smallpox inoculation. Mr. Earle, writing to Dr. Jenner,* says, in March 1784, "I inoculated several patients with active variolous matter, all of whom had the disease in a favorable way; but, my matter being all used, and not being able to procure any more in the state I wished, I was under the necessity of taking it from a pustule, which experience has since proved was advanced too far to answer the purpose I intended. Of five persons inoculated with this matter, four took the smallpox afterwards in the natural way. . . . I conceive the appearances were such as might have induced any one to suppose that the persons were perfectly safe from future infection. . . . About the ninth day eruptions appeared, which died away earlier than common without maturation." He mentions then some similar instances in which he performed inoculation a second time with well-selected virus, and obtained a full and perfect result.

4. *The manner in which the pock passes through its several stages.*

It was an early opinion among vaccinators that interference with the course of the vesicles of vaccine was one of the causes of the want of protective power exhibited at a later period. Hence it became customary to leave one vesicle, at least, to pursue its course, without being opened to obtain a supply of lymph. The practice is a good and a prudent one; but, nevertheless, it behoves us to consider how far mechanical interference with the course of the vesicles damages, or is likely to damage, the protective quality of the vaccine disease. It is not uncommon for injury to be inflicted upon vesicles by rubbing or scratching; and, when their progress is thus interfered with, it can well be understood that the resulting scars will not present their proper normal character. Now, there are two considerations con-

* Jenner, 'Further Observations,' &c., p. 11.

needed with the early practice of vaccination, which are calculated to lead to the conclusion that, at a certain stage of the vaccine disease, such injury to the vesicle may not damage very materially its protective power. I have already mentioned that at first it was the practice with vaccinators only to make a single puncture, and hence the necessities of transmission demanded in many, if not in most instances, that this vesicle should be punctured from the sixth to the eighth day. Yet those vaccinations were protective, as shown by the inoculation test as applied by Jenner and his contemporaries. The other point is that it was a practice with some of the early vaccinators absolutely to destroy the vesicle at a certain stage of its progress by the use of a caustic. The object of this was to prevent an apprehended inconvenience, the troublesome spread of local inflammation. Dr. Marshall, in his second letter to Jenner,* states that it was his practice, when the disease had in his opinion duly acted upon the constitution, to cauterize the pustule with a drop of vitriolic acid; yet every one of his subjects, to the number of 211, resisted the subsequent inoculation of smallpox matter. Dr. Jenner speaks favorably of this practice, but adds,† “I would not recommend any application to subdue the action of the pustule until convincing proofs had appeared of the patient’s having felt its effects at least twelve hours. No harm could, indeed, ensue were a longer period to elapse before the application was made use of. In short, it should be suffered to have as full an effect as it could, consistently with the state of the arm.” He narrates the result which followed in one instance in which he destroyed the pustule, as it appears, somewhat prematurely. This was the case of a child aged seven years, in whom “the virus on the arm was destroyed (by caustic) soon after it had produced a perceptible sicken-

* ‘A Continuation of Facts and Observations,’ &c., 1800, p. 16.

† ‘Further Observations,’ &c., p. 37.

ing." This was in April, 1798. In December smallpox inoculation was performed, and the child was also exposed to smallpox contagion. The smallpox virus took locally in the customary manner; but on the ninth day, when the general eruption was anticipated, nothing occurred but some feverishness and a fugitive rash about the wrists. In this instance it is clear that a complete protection was not given. Bousquet states* that he himself has purposely opened the vaccine vesicles on their appearance, and cauterized them deeply, and yet after this revaccination has failed. He does not appear to have experimented by way of revaccination after the lapse of some months or a few years, nor do we know anything of the subsequent history of these cases. I should myself be very apprehensive that the capacity for development destroyed, for the moment, at the time that the vesicles began to form, would not be permanent, and that, on exposure to smallpox inoculation or contagion, a similar result to that in Jenner's case would have followed. I have already insisted upon the fact that the amount of protection given by vaccination is dependent upon the perfection of the disease as an exanthematous malady; and certainly it cannot be said to be perfect in this sense until evidence of constitutional disturbance is afforded by the appearance of the areola. After the eighth or ninth day I infer that the perfection of the vesicle is a matter of little importance, for from this time it begins itself to undergo destructive changes, the lymph becoming turbid and the process commencing, which finally terminates in desiccation; but prior to the eighth or ninth day, so long as the lymph is clear, it appears to be very important to protection that the course of the vesicle should be naturally pursued.

5. *The source from which the lymph for vaccination is derived.*

* Op. cit., p. 302.

I have already spoken of lymph as taken from imperfect pocks, from pocks too far advanced, and so on; but what I am about to discuss now is the question whether good lymph, taken from a healthy and normal pustule at the proper time, differs in its effects upon the system according to the number of human generations it has passed through since it was first taken from the cow. It is a question which has been much discussed, and upon which the most opposite opinions have been held. It requires, therefore, from me a full inquiry in this place. I ask, then—

Is there any satisfactory evidence that vaccine virus, in its transmission through successive human generations, loses any of its activity or becomes less effectual, when developed in the system, as a protection against smallpox?

This is a very difficult question to answer. *Primâ facie*, we might say, that, as vaccine is a disease unnatural to man, a disease of an animal lower in the scale of organization, implanted upon man by art, it is not improbable that some careful cultivation would be necessary to prevent its degeneration. If we look to the analogies of the vegetable world, we certainly come across notorious facts which would prepare our minds to receive the doctrine that such deterioration really takes place, and which demonstrate the degeneration which many plants, the cultivation of which is practised by man, undergo in process of time, when they are grown on soils or in climates unnatural to them, and the necessity which arises consequently, from time to time, of obtaining fresh seed from their primary and natural habitat. A similar difficulty is met with in the breeding and acclimatisation of foreign animals. Even man himself undergoes modifications in stature and appearance, as well as in mental characters in the course of successive generations, when transplanted

from one region of the world to another. Still such analogies prove nothing, they only serve to prepare us for the reception of proof, by showing that the event would be one not out of accordance with the order of nature.

The general *analogies of disease* have also been appealed to. It has been said that certain virulent disorders have, in course of time, died out of countries; or, if not so, have become lessened in severity in the course of years. Leprosy is one of them, a malady which is said to have committed very serious ravages in Europe in the fourteenth and fifteenth centuries. Syphilis is another which is said to be less violent in its effects than it used to be, and even smallpox itself is appealed to in proof of the same truth. The facts I am not prepared to deny; but in the facts I see nothing that is capable of assisting the argument based upon them. Leprosy, syphilis, smallpox, the black death, sweating sickness, plague, &c., may all have disappeared or assumed a milder form among Europeans. But all this may be accounted for by other considerations, such, for instance, as the very different mode of life now to the time when the earthen floors of English dwelling-houses were strewed with rushes, while the *débris* of former strewings were left to decompose beneath the fresh one, when land and house drainage were things unknown, and when fresh meat and fresh vegetables were denied, except to the very few, during the winter months of every year; when medical practice was unenlightened by an acquaintance with the anatomy and physiology of the body, and when proceedings were adopted in the management of disease which we now know assuredly would augment its severity and promote its fatal termination.

Although with regard to smallpox we must admit that the transmission of the disease, within the historical period, has taken place from generation to generation without any material reduction in its natural virulence, yet there are facts to

show, not only that the disease may in certain constitutions be modified in its manifestation, but that, when modified to a certain extent, the virus itself may become degenerated in its characters. M. Lafont-Gouz* relates that, at his request, his colleagues at the Hôpital la Grave at Toulouse inoculated with varioloid matter several foundlings who had neither had smallpox nor cow-pox, and produced in them an eruption equally equivocal as that from which the matter was derived. In the next generation, however, the virus was found to have recovered its energy, for, on inoculation from these subjects, it produced a smallpox eruption with well-marked characters, and the disease spread in the hospital in a manner which put the recovery of the virus beyond question. Guillon and Sacco have also made successive inoculations with varioloid virus, and have found that it sometimes passed through two or three generations before the energy of the smallpox virus was restored. There was then a true degeneration, which indeed recovered itself after a time, but still was at first transmitted as a degenerated virus from subject to subject.

To pass on to vaccinia, I have said that experience has demonstrated that the disease, when imparted to certain subjects, produces in them an imperfect or modified development, and that lymph taken from such pocks or from vesicles which are too old or damaged has so far lost its characters as to give rise, when introduced into the system, to an imperfect vaccine disease. There is, then, in vaccinia a degeneration of virus taking place under certain circumstances; and we can understand that, in some localities where vaccination is carelessly performed, this may seriously damage protective power. That in successive generations the qualities of the virus may not recover themselves, while those of modified smallpox virus may recover themselves, is no more than

* Bousquet, *op. cit.*, p. 207.

can be explained by the fact that the latter is a disease natural to man, finding in him its normal habitat, whereas vaccine is a disease foreign to him and merely transplanted upon him from a lower animal.

But it is not this which is meant when people talk about the degeneration of the vaccine virus. It is said that the virus degenerates by successive human generation even when the utmost care is taken in the selection of the vaccinifer, and in taking lymph from poeks of the proper age. Is this so? I believe that it is—and in stating the arguments which have been brought forward in favour of the doctrine, I shall endeavour to show which are fallacious and which are valid, giving to each what appears to me to be its proper value.

Although Jenner had, from an early period, recommended that recourse should be had anew to the cow as a source of vaccine lymph, as frequently as possible, it was not before the year 1814 that the *opinion* was distinctly put forward by Dr. Kinglake* that the vaccine virus gradually its lost power by successive human transmissions, and that he recommended that fresh lymph should be taken as often as possible from the cow. In 1818 this opinion was gaining ground to such an extent upon the Continent, that the government of Wirtemberg endeavoured to provide for a periodical renewal of the virus, by directing that a certain number of cows should be vaccinated annually. In the same year, M. Brisset gave the weight of his authority to the opinion, in which he was followed in 1823 by our countryman Dr. Gregory. Since then various writers have promulgated similar views, but it is only since the year 1836, when a number of comparative experiments were made, under the auspices of the Vaccine Committee of the French Academy, that a solid basis for the opinion has been obtained. A few years prior to this, M.

* 'Med. and Phys. Journ.,' vol. xxxii, p. 180.

Bousquet, in his celebrated work which I have often had occasion to refer to, strenuously maintained the opposite view; but the observations which he made in 1836, with the lymph obtained from a new source, discovered at Passy, led him to its complete abandonment. I may arrange the arguments put forward from time to time by those advocating the notion of progressive degeneracy as follows :

1st. I may first of all dispose of an argument which is utterly valueless, but which was put forward by M. Nicolai in 1833. The view he adopted was that the atom of *vaccine virus* introduced into the system became greatly *diluted by the humours of the body in the formation of a new vesicle*, and that, as the transmissions were continued, each successive generation would produce a virus more and more diluted, the dilution becoming greater and greater, in arithmetical ratio, until, by the time the thirty-fifth generation was attained, the quantity of true cow-pox virus in a vesicle would not constitute more than $\frac{1}{809458688}$ th part of what it constituted in the first generation. I think I have stated his idea correctly, but whether I have or not is of very little consequence, as the whole theory is based upon a fundamental error, inasmuch as he has mistaken entirely the pathological process by which fresh vaccine virus is generated from the blood in the formation of the vaccine vesicle. The theory is equally worthless with the fallacy on which it is based.

2nd. The next class of arguments I shall notice is that which is founded upon *observations as to the altered characters which the vaccine vesicle and the vaccine disease*, as a whole, have been believed to exhibit when compared with those which they presented at earlier periods of the practice of vaccination. The smaller size of the vesicle, the less extent of the areola, the less degree of vaccinal fever, and the comparative infrequency of any affection of the axillary glands, as

observed now, when compared with the descriptions and delineations of Jenner and the recollections of former times by some observers, have been much dwelt upon by nearly every person who has written upon the subject from the time of Brisset onwards. Lüders, writing in 1824, notes especially the great difference which he himself observed in the vaccine vesicles which he obtained at that time, from those which he obtained from the use of the virus eleven years previously. Mr. J. B. Estlin in 1838* says—"The alterations in the vaccine affection which have appeared to me most marked are the smallness of the vesicle and its attendant areola, its rapid course, the absence of constitutional disturbance, the small quantity of lymph yielded by the vesicle, and especially the diminished activity of its infecting quality. Twenty years ago it was a matter of comparative indifference how long the lymph taken was kept before it was used . . . at the present time virus a day or two old is very uncertain in its effects, and even fluid lymph often fails to communicate infection. On the diminished anti-variolous powers of the present stock of vaccine matter I need make no remark, the public are too painfully aware of the fact." Again, it has been said that the scars left by vaccination are less complete and less strongly marked than they used to be formerly. Thus, Drs. Medicus and Oegg,† during an epidemic of smallpox near Wurzburg in 1825, examined the scars of vaccination throughout the village where it occurred, and noted that the cicatrices of those vaccinated in 1802—1804 resembled the scars left by burns, being irregular in shape, and the size of a half-franc piece, and that they were unequal in depth, and striated. On the other hand, in those persons who were vaccinated from 1805 to 1812 the cicatrices were only half this size, while in those vaccinated at subsequent periods they were

* 'Med. Gazette,' vol. xxii, p. 977.

† Quoted by Steinbrenner, p. 495.

still smaller, of the size of a lentil, and not much below the level of the skin. I do not know that very much value attaches to this observation, because such a degeneration of virus as this statement would involve is greater than experience warrants us in believing to be likely, unless where vaccination had been conducted with carelessness in the selection and propagation of the virus. The experience of Dr. Duncan Stewart* appears to show that the vaccine virus had certainly degenerated between the time when vaccination was introduced into India and the year 1839, when he obtained a fresh supply of lymph from England. He thus describes the difference he and others to whom he distributed the new supply observed in its operation from that of the lymph current at that time :—"The peculiarities which I note in the new stock, as compared with the old one, are—1st. The latent period intervening between the performance of the operation and the development of its specific effects is much longer, extending from three to six days before the puncture begins to inflame at all; and, during this period, so inert has the virus frequently appeared that parents have frequently reported its total failure in cases which so late as the seventh or even eighth day have budded, and progressed to great perfection. 2nd. The vesicles are larger and more globular, they contain clearer lymph and more of it, their disc is more exact and circular, and the central depression exists from the first. 3rd. The areola is uniformly much larger and broader, and in European children the colour, firmness, &c, are strictly Jennerian. 4th. The constitutional fever is considerable on the eighth and ninth day, and even when not so is always well marked. 5th. The course of the disease is altogether slower, more deliberate (if I may say so), than was the case generally with the old one. The vesicles often do not begin to become opaque until

* 'Op. cit., p. 155.

the tenth or eleventh day, and the crust never falls off before the thirteenth or fourteenth day." With such a description of the improvement effected by the importation of a new supply of lymph we may gather readily an idea of what the vaccine disease had become in India prior to 1839.

3rd. M. Bousquet mentions a series of comparative experiments made by M. Rigal de Gaillac, who performed *vaccination with vaccine crusts which had been preserved from different periods*, some being the products of vaccination performed in 1813, others of vaccination performed in 1817, and some taken only a few years before the crusts were used. A difference in the results was observed in favour of the older crusts, especially in respect of the rapidity and certainty of the effect and the characters of the vesicles. But the experiment was wanting in some necessary elements of precision, because all the crusts had not been preserved with equal care, and even had they been so, the observations were too scanty to warrant any inference that could be relied upon.

4th. Steinbrenner mentions, among some Wirtemberg *observations*, an event which appears to show that *under certain circumstances a rapid loss of power may take place in vaccine virus*. Dr. Straub, of Gmünd, vaccinated two children successfully. The lymph furnished by the one produced very fine vesicles; while that furnished by the other, although in the first generation normal vesicles were produced, failed to preserve its powers, and could not be propagated at all beyond the second generation. We do not know all the circumstances of this remarkable case; it is probable that if we did we should find the rapid loss of power to have been capable of explanation by some such unusual accumulation of unfavorable coincidences as is sometimes observed in medical practice under other circumstances, and which overthrow

completely all the anticipations which prior experience may have led the physician to form.

5th. One of the grounds on which an early opinion as to the probable degeneration of the vaccine virus was based, was the *growing frequency with which varioloid disease and true smallpox were observed in vaccinated persons*. Such an occurrence, however, is readily explicable by the greater extent to which vaccination year by year has been performed, and by the fact of the wearing out of the protection in a proportion of vaccinated subjects by progress of time. It was and is a sufficient reason why an inquiry into the continued protective power of the vaccine virus should be instituted, but in itself is no proof whatever of its loss or deterioration.

6th. Another argument adduced is that *revaccination of adult subjects is more successful than it used to be*; that there are fewer failures, and that the result of the operation is more complete. Thus, Mr. Marson writes in 1853* :—"For many years past I have practised revaccination extensively on persons applying for the purpose at the vaccination-room, on the servants and nurses of the hospital, on persons coming to visit their friends, patients in the hospital, and lately on the numerous workmen employed in building the new hospital. The effect produced by revaccination sixteen or seventeen years ago was, with some few exceptions, nothing more than a little irritation, or at most an abortive vesicle with irregular areola. But during the last three or four years I have seen a great many persons on whose arms the vesicles produced by revaccination have been quite or nearly perfect, even on those who bore good cicatrices from the first vaccination." The experience of revaccination in the Prussian army, regularly published in the 'Med. Zeitung,' has been appealed to in evidence of a progressive increase in the success met with, and of a proportionate resusceptibility of vaccination, and so,

* Marson, loc. cit., p. 385.

presumedly, of a capacity for developing smallpox virus at a given age. And if the capacity for developing the virus has increased year by year, we must infer that a proportionate increase, probably, has taken place in the number of vaccinated persons who are liable to receive the virus on exposure to contagion. "It is difficult," says Mr. Simon,* "to conceive how the infantine generations of a country could, crop by crop, successively derive less permanent constitutional impressions from vaccination, unless the efficient cause of those impressions, the vaccine contagion itself, had year by year undergone enfeeblement of its powers." Mr. Simon goes on to say:—"In Prussia the vaccination of recruits is a very uniform test. It extends annually to some 40,000 or 45,000 operations. It is reported on annually. Its records run back for twenty-four years. Its total result must tell of a million experiments; and the subjects naturally are of like age, in like proportions, and under like circumstances. I have already had occasion to say that when, in 1833, this system of revaccination commenced, the proportion of successful results (including cases in which the success came only with a second attempt) was thirty-three in every hundred vaccinations. Now, the annual per-centages of successful results for the whole time during which revaccination has been practised in the Prussian army, beginning with that number, run thus:—33, 39, 42, 46, 49, 50, 51, 54, 57, 58, 57, 57, 58, 60, 64, 64, 64, 61, 64, 69, 69, 69, 69, 70. *The last proportion of success exceeds the double of that with which the series commenced.* Thirty-three per cent. expresses the proportion in which persons vaccinated, say, twenty years previously, had, in 1833, to a certain extent lost the influence of their infantine vaccination; it measures the impermanence of certain impressions produced by the vaccinations of 1813. And that impermanence (such as it was) in the

* 'Papers,' &c., p. xli.

effects of vaccination has increased, almost without exception, year by year, during this quarter of a century; *so that the vaccinations of 1836, tested by eventual resusceptibility to cow-pox, were not half so stable as the vaccinations of 1813.*"

There can be only two explanations of this remarkable fact—the one, that the virus has degenerated in its protective power, as the result of frequent human transmissions; the other, that there has been a steadily progressive carelessness in the mode in which primary vaccinations have been performed, and that this carelessness has resulted, as I have shown it may result, in a deterioration of the virus. From one cause or the other it is very clear that, in Prussia, the virus has been gradually losing its protective efficacy. With regard to the latter of the causative alternatives, common experience has shown that, as vaccination is pushed forward among a population, so as to increase in quantity, it is very apt to suffer in quality. I myself found that, in 1863, when I had occasion to urge on the vaccinations among a large population, in consequence of the epidemic outbreak of small-pox, recourse was had very largely to dry lymph, and the demand for "points" was so great that they were charged from almost any subject capable of supplying lymph. Many hundreds of such charged points came under my supervision, and I had occasion to reject a considerable proportion on account of their being tinged with blood. The emergency was so great, and the demand for an immediate protection was so extensive, that any points which I could supply were greedily taken into use. Now, what occurred at that time was but a sample of what may fairly be expected to occur whenever private and public vaccination come suddenly into requisition. Every particle of virus, whatever its quality, becomes precious; and the object is to produce a pock of some kind from which other persons may be vaccinated. In public vaccinations, again, where the poor have to be visited,

where they will not take the trouble to attend at the residence of the vaccinator, it is a very common thing to use dry lymph as the most convenient method of carrying the virus; and the more public vaccination in large towns is stimulated, the more vaccination is conducted in this manner, and the more likely it becomes for a comparatively ineffective virus to be perpetuated.

The experience of revaccination in the British army does not confirm the Prussian observations; but the circumstances of recruiting in the two countries may be so different as to be capable of accounting for the discrepancy in the result of the operation. Thus, the complete failures in adult vaccination of British recruits, taking all together in consecutive years, were per 1000 as follows:—1858-9, 384; 1860, 296; 1861, 342; 1862, 340; 1863, 377; 1864, 306. And if we confine ourselves to an examination of the vaccinations made upon those only who had good scars on their arms, we find the failures per 1000 to be in the same series of years 390, 311, 351, 351, 393, 311. Probably this irregularity of result may depend upon the varying proportion in which the recruits are drawn from different parts of the United Kingdom, and the varying degrees of goodness in the vaccination in different localities. In England it is certain that this differs very greatly.

7th. But the principal evidence which we possess of the deterioration which the vaccine virus undergoes in the course of frequent human transmissions is that which arises from *comparing the effects produced by selected lymph which has undergone numerous human transmissions with that produced by lymph recently derived anew from the cow, or after having passed through only a small number of human generations.*

On various occasions, since a suspicion arose as to the deterioration of the vaccine virus, opportunities have presented themselves of obtaining a renewed supply of lymph from the

cow or from persons naturally infected with the disease. I may pass over the earlier observations made with lymph thus collected (a condensed account of which will be found in Steinbrenner's work and in Bousquet's pamphlet, and some of which are open to criticism), in order to arrive at the year 1836, when a new and important source of lymph was opened in France by M. Bousquet, and another two years later at Bristol by Mr. Estlin. I will commence my remarks with an account of the former, inasmuch as M. Bousquet's observations were fuller and more precise than those published by our countryman, and have received confirmation at the hands of another very accurate observer, M. Steinbrenner.

*Observations of M. Bousquet with Passy lymph.**—On the 21st of March, 1836, a woman residing at Passy, a milker, applied to a medical man in her neighbourhood on account of an eruption upon her hand. He made some inquiries of her, and recognised the affection as the vaccine disease. The same day he sent her to M. Bousquet. That observer states that there were three vaccine pustules upon the right hand and one upon the upper lip, and he was struck by the unusual bluish tint they exhibited, such as Jenner had mentioned as a characteristic of true cow-pox. From a pustule on the hand M. Bousquet vaccinated nine children. The lymph taken, however, was late in the course of the disease; it was thick, white, and purulent. Still he used it, such as it was, vaccinating the children with it by three punctures on the left arm, and using the current lymph in three places upon the right arm. In one of these nine children no effect followed on either arm; the child was only three days old. In the other children the old lymph took, and from twenty-four punctures there were obtained twenty-two pocks. But in only three of these eight children did the new lymph take upon the left

* 'Sur le Cow-pox, découvert à Passy le 22 Mars, 1836.'

arm, and in each of these it took only in one place. The ages of the children were severally ten, seven, and three months. We observe, then, that at this early stage of human transmission there was some difficulty in generating the virus. Only one of these three children returned for examination on the eighth day. Denis was the youngest of the three, a weakly, puny infant, and all the punctures, the three upon the right arm with the old lymph and one upon the left arm with the new, had given rise to pocks as miserable and languishing as the child itself, and of the two arms the left was the least satisfactory. However, M. Bousquet took lymph from it, and vaccinated four other children on the left arm, again making a comparative vaccination with the old lymph upon the right arm. In the two other children the product of the new lymph was, indeed, not so shabby as the vesicle on the arm of Denis, but still it was nothing remarkable, being quite similar to that upon the right arm from the lymph in current use. This, then, was the *second human generation*, counting from the cow. Two infants were vaccinated from the last-mentioned infants, but the old lymph was not used upon the other arm. In describing, then, the results of the *third generation* I shall confine myself to the account given of the four children vaccinated from the single poor vesicle on the infant Denis. All the punctures took, the three with new virus on the left arms and the three with old virus on the right, in all the four children. The vesicles ran a completely parallel course up to the seventh day, when a difference became perceptible in favour of the new virus. The vesicles were better formed, flatter, and more depressed in the centre on the eighth day than those from the old virus, more brilliant in appearance and firmer, and the lymph they contained was as clear as crystal. On the twelfth day the difference was still more marked; the new vesicles were flat and nearly four lines broad, firm at the border, prominent and full of

force and vitality; the old ones, on the other hand, were already beginning to dry up. M. Bousquet, no doubt, correctly attributes the less perfect character of the vesicle of the second generation to the old and purulent condition of the matter when taken from the woman Fleury's hand, "but, at bottom, the quality of the vaccine was excellent."

M. Bousquet's summary of the different results obtained from the use of the new and the old lymph in the numerous comparative vaccinations he performed is briefly as follows, nearly in his own words:—The new vaccine proceeds in its course at once more quickly and more slowly than the old—more quickly in that it sooner gives signs of life, more slowly in that its career is more prolonged. The resemblance between the results of the two sources of virus is at no time greater than on the seventh or eighth day.* From that period the vesicles are quite distinguishable the one from the other and the more they advance the more pronounced becomes the separation. As to size, it is no exaggeration to say that those of the new vaccine often acquire a size almost double that of the others. M. Bousquet has seen them so large that a ten-sou piece would not cover them; they are also flatter, more brilliant, more umbilicated, better defined, and firmer. When looked at closely, they present to the eye a grained surface, something like that of the rind of an orange; they appear to be more closely and firmly attached to the skin, and as they become more developed they raise and drag upon it; while the old vesicles are slighter, more superficial to the eye, and are raised, and look as if separate from the skin, more after the manner of a "vesicle." Corresponding to this is the areola;

* This fact of the similar course of vesicles from old and new virus up to the eighth day explains the contradictory opinion put forth by various persons, that the results of using new and old virus are perfectly similar. This would be the inference naturally drawn if the subject were only seen on the eighth day, as is the custom with vaccinators.

in the one it is active, extended, and phlegmonous; in the other slight, fugitive, and erysipelatous. The areola appears also sooner around the old than around the new vaccine, by an interval of one or two days. At first, M. Bousquet, after his usual habit, made three punctures with the new lymph, but he soon had to renounce this practice, as the inflammation was sometimes so great as to affect the whole arm and the axillary glands. He says that M. Gasc has never been able to forget one infant that he vaccinated; the pustules were enormous; the inflammation so violent that baths, poultices, fomentations, and an antiphlogistic regimen scarcely sufficed to calm it; and when the crusts fell off they left an ulceration which took a long time to cicatrise. From this moment, says M. Bousquet, "I comprehended for the first time the fears of Jenner, who dreaded inflammation so much that he only made one puncture on each arm," and even then, as soon as he thought the system to be sufficiently impressed, took measures to arrest the progress of the local affection, and even went so far as to destroy the pustule with caustic. He adds, "There is nearly as great a difference between the course of the two eruptions as between 'variola' and 'varioid.'"

As further evidence of the greater power of the new lymph M. Bousquet states that, on an equal number of persons vaccinated with the new and the old virus, he found that the old virus gave him 628 vesicles and the new 776, making thus a difference of 148 vesicles in favour of the new source; and this notwithstanding that he habitually made three punctures with the old and only two with the new lymph upon the arm. He says that he also succeeded in obtaining results when taking the new virus from vesicles at a period as late even as the eleventh and fifteenth day. Another proof of energy is found in the fact that he vaccinated thirteen soldiers, some of whom said they had had smallpox, while the

others had been vaccinated previously, and that in all thirteen he obtained a number of pustules equal to that of the punctures which he had made. This result is by no means a common one, as we have already had occasion to see ; probably, had the old virus been employed, half of these men would have presented no result of the vaccination at all. He made other vaccinations, and obtained successful results in six or seven out of fourteen or fifteen individuals.

M. Boucher, of Versailles, also made some comparative revaccinations upon twelve persons, using the new lymph upon the one arm and the old upon the other. He found that the new produced superb pustules on all of these individuals, equal in number to the punctures, while the old, if it took at all, gave only modified or false pustules. Several medical men experimented subsequently with the new lymph, and their observations confirmed those of M. Bousquet.

*Observations of Estlin with new source of lymph at Bristol.**

—Mr. Estlin heard, on August 18th, 1838, of the appearance of cow-pox in a farm in Gloucestershire, where several of the cows were affected, and where the farm servants had received the contagion. All those attacked had, by the way, been vaccinated in infancy, and one of them by Dr. Jenner himself. By the time Mr. Estlin visited the farm several children had been inoculated with a needle from those who had taken the disease. Estlin describes the eruption on them (the *second generation*) as large, firm, and prominent ; and that the eruption in one was delayed is presumable from his having been informed that for three or four days it was difficult to decide whether the vaccination had taken. He took away with him from the farm some lymph taken directly from the cow, lymph from the hand of a boy who had received the contagion (*first genera-*

* 'Medical Gazette,' vol. xxii, p. 977, "Account of a supply of fresh Vaccine Virus from the Cow."

tion), the vesicle being six days old, and lastly some lymph from a girl who had been vaccinated from one of the servants (lymph of the *second generation*). The lymph from the cow and that of the first generation both failed when used in vaccinating other children; but that of the second generation, from the girl's hand, succeeded upon two subjects out of several whom he vaccinated with it. It is to be observed that it was carried away upon glasses, and would probably have succeeded better had it been used fresh. He says that one of these two subjects had a single vesicle produced, the other had two. In both subjects the disease was late in coming on; in one of them no redness appeared at the base of the vesicles until the tenth day, and the areola was not fully formed until the thirteenth day. In this case, however (Sarah Owen's), each vesical was very perfect, rising abruptly from the arm, its upper part almost overhanging the base; its surface was much flattened, and it yielded freely limpid fluid when punctured before the areola appeared. On the thirteenth day the child's body and extremities were covered with a rash in patches, much elevated from the skin, and she was constitutionally indisposed. On the fifteenth day the surface of the vesicle was becoming brown, and the areola, rash, and general indisposition, had disappeared. In this description, although less precise than that of M. Bousquet, we recognise many of the characters of the vaccine disease as proceeding from the use of the Passy lymph of the same (the *third*) generation. From a report forwarded to Mr. Estlin by a Vaccine Committee of the Faculty of Medicine at Glasgow,* it appears that, as with Passy lymph, the successes in vaccination were more common than with the old lymph. Out of forty-three trials with the new lymph there was not a single failure, whereas, on a similar number of vaccinations made with the old lymph in current use just previously, there were

* 'Medical Gazette,' vol. xxiv, p. 208.

ten absolute failures, and in nine instances the pocks were spurious or imperfect.

*Gregory's observations with a renewed lymph at the Small-pox Hospital.**—The source of the new lymph is not mentioned, but the difference in result from the lymph in use at the hospital may be given in Dr. Gregory's own words:—"The lymph in use at this hospital has been preserved in uninterrupted descent for a very long period of time; but for three or four years past I had noticed that its intensity was diminished, and that eight or ten incisions produced not more irritation than the three to which I was accustomed fifteen years ago. In March last (1836) Mr. Marson, the resident surgeon, employed lymph obtained from a different source. This new lymph was found to be more intense and active than the old lymph. Three or four incisions were now found amply sufficient; and so satisfied was I of the superior quality of the new lymph that, after a careful trial of about two months, the old lymph was suffered to die out, and for the last six months we have vaccinated exclusively from the new stock. These facts have convinced me that vaccine lymph, in passing through the bodies of many persons, loses in process of time some essential portion of its activity. It follows from this that an occasional resort to primary lymph from the cow becomes a matter of great importance, perhaps even of indispensable necessity."

Observations of Steinbrenner with the Passy lymph and with new lymph obtained from other sources.—Steinbrenner commenced his observations, just as Bousquet did, with a leaning towards the doctrine of unalterability in the quality of the virus. His first comparative experiments were made with the Passy lymph, which he received on October 20th, 1840,

* 'Medical Gazette,' vol. xxi, p. 861.

and which had, therefore, undergone above four years' human transmission, and so was lymph of about the 240th *generation*. It is important to notice this. The old lymph which he used was derived from the virus forwarded to his father by Dr. Woodville in 1802, and the supply of which had been kept up by him and then by his son uninterruptedly ever since. This lymph, then, must have passed through about 2000 human *generations*. His first object was to ascertain whether M. Bousquet's method of experimenting by vaccinating old and new virus upon the same subject was worthy of reliance, or whether the presence of the one virus in the system modified the operation of the other. He therefore vaccinated three infants, one with the old virus upon the right arm; a second with old virus on the right arm and new virus on the left, after the manner of Bousquet;* and the third child with the Passy lymph upon both arms. The result was that the pustules on the right arm of the second child ran precisely a similar course to those on the arm of the child vaccinated with the old virus alone, and that the pustules on the left arm ran a precisely similar course to those on the arms of the third child, vaccinated from the new source alone. The inference was that the operation of the one virus did not modify that of the other, and thus an additional confirmation was obtained of the value of the previous comparative observations made by Bousquet and others in 1836. M. Steinbrenner gives us, in parallel columns, the comparative results obtained. I will take them only from the eighth day.

* In making these comparative punctures both Bousquet and Steinbrenner employed a perfectly new lancet from the cutler's shop for inserting the new virus. This was a wise precaution, inasmuch as Woodville's observations were damaged in value by the use of a lancet which had been formerly employed in smallpox inoculation, and which had only been newly ground.

8TH DAY.*

Old vaccine—right arm.

The pustules appeared the same as yesterday, only perhaps a little larger and *less* brilliant. A second pustule, when opened, furnished a *lymph already slightly clouded*. All the pustules are surrounded by areolæ, which, however, are not yet complete except around two of them.

New vaccine—left arm.

The pustules are still *more* brilliant than yesterday, they are also larger, and are now raised so as to form completely a *right angle with the skin*. The inflammatory redness is more extended and more pronounced, and shows the approach of the formation of the areola. Two of the pustules being opened furnish a *lymph clear as crystal*.

9TH DAY.

The pustules are surrounded by well-marked areolæ, with a radius of about 10 or 12 millimeters. They are larger, higher, softer, and are *becoming yellow in colour*. The central depression has disappeared, and in its place there is a light brownish crust, which indicates the commencement of desiccation.

The pustules are large and fine; they have *not yet lost their silvery appearance*; one of them being opened gave issue to a *lymph quite as transparent* as yesterday. The areolæ are in course of formation, but are *not yet complete*.

10TH DAY.

The areolæ have become pale, and are *half effaced*. The brownish crust in the centre is increased in extent, the pustules are lessening, and every thing announces that *desiccation is in full progress*.

The areolæ are to-day *well developed* and well marked; one touches another; they may have a radius of from 15 to 18 millimeters, are very red, and seated upon a deeply engorged base, and are more elevated than the rest of the skin of the arm. The *whole arm is hot*, and two small glands are to be felt distinctly *engorged in the axilla*, whilst nothing of the sort is perceptible on the other side. The child is restless, with a fuller and more quickened pulse for the last few days. During one night there was more restlessness, and the child cried a good deal. However, there is no pallor of the face perceptible, neither is there any evidence of internal congestion.

* Steinbrenner, op. cit., p. 537.

11TH DAY.

Old vaccine—right arm.

Every trace of *areolæ* has disappeared, and all the pustules are covered with a yellow crust, which increases in extent as the pustule fades and becomes smaller.

New vaccine—left arm.

The *areolæ* are still fine, the pustules are yellowish and have lost their brilliancy. The *glandular engorgement* is more considerable, and there is apparently much tenderness on pressure over them.

12TH DAY.

All the pustules are dried up and covered with a crust, which is still soft and of a brownish-yellow colour; the skin about them is only slightly engorged and reddened.

The eruption is now in full development. The pustules are fine and large, with the diameter of a centimeter (whilst the pustules on the right arm when at their largest—on the ninth day—did not measure more than 7 millimeters). A delicate crust of a mahogany colour begins to take the place of the central depression; the edge around the crust is prominent and of a dull yellowish-white colour. The *areolæ* are still very vivid, and have lost none of their extent.

13TH DAY.

The crusts have become harder, browner, and smaller.

The central crust increases in size and thickness, and, as it extends, the surrounding part of the pustule becomes smaller, but is otherwise only a trifle softer.

14TH DAY.

The crusts are quite dry, hard, brown, of 2 millimeters in thickness, and 3 or 4 millimeters wide.

The desiccation continues, the pustules are more withered, the crust increases in breadth and thickness and covers almost all the surface of the pustule. The *areola* begins to become pale, and the subjacent engorgement is less considerable.

15TH, 16TH, AND 17TH DAYS.

Old vaccine—right arm.*New vaccine*—left arm.

The pustules are all drying up and becoming covered with broad thick crusts of a deep mahogany colour, or like that of a roasted almond (as M. Bousquet describes it). The areolæ have completely disappeared since the sixteenth day.

17TH TO 19TH DAY.

The *crusts* fell off, and left reddish superficial depressions.

Three weeks later, the redness having disappeared, rounded *cicatrices* of the size of a lentil, depressed below the level of the skin, and dotted with eight to twelve small points still more depressed, are observed. These *cicatrices* differ little in colour from that of the surrounding skin (they are only a little whiter), and at some distance are scarcely perceptible.

18TH DAY.

The *crusts* are very hard, and 2 to 3 millimeters thick, and from 5 to 7 broad. They *still adhere firmly* to the subjacent parts.

23RD TO 26TH DAY.

The crusts fell off and left *cicatrices* as broad as themselves, deep and crossed with bands, and with furrows, at the bottom of which were a great number of small points.

Three weeks later, the redness having disappeared, the *cicatrices* were found of a white colour, and reticulated and dotted over with points at their base. They are very distinct, and can be readily seen at a long distance.

Prior to the eighth day the products of the new and old virus, as observed by Bousquet, ran a similar course. It cannot fail to be observed how thoroughly this parallel confirms the prior observation of Bousquet as to the difference in the course of the two eruptions after the seventh day from vaccination. With regard to the *constitutional reaction* arising from the use of the new and old virus, Steinbreuner found that in none of the children vaccinated with the old virus was there ever any engorgement of the axillary glands; whereas out of sixty of those vaccinated with Passy lymph

this phenomenon appeared in twelve. In only five of those vaccinated with old virus was there acceleration of the pulse on the eighth or ninth day, with a slight increase of bodily temperature and a little restlessness; whereas in sixteen of those vaccinated with Passy lymph there was observed from the ninth to the eleventh day well-marked febrile disturbance, with full and accelerated pulse; the heat of the skin, especially of the arm, was increased; there was little sleep and a good deal of crying; in fact, everything to point to a general affection of the system, such as is scarcely ever seen when old lymph is used. The *fall of the crusts* when old virus was used took place from the fifteenth to the twenty-fifth day (mean, eighteenth day); when Passy virus was used it took place from the twenty-first to the thirty-fourth day (mean, twenty-sixth day). The *cicatrices* left by the old virus were round, slightly concave, pitted at the base, of a rather whiter tint than the skin, and scarcely tender to the touch, the size of a lentil or a little larger; some of them formed only slight whitish depressions, not pitted at all, and in some instances there were only whitish stains, to which the term "cicatrix" could scarcely be applied at all. In only three instances were the cicatrices larger, deeper, and traversed by white hard bands. The scars left by the Passy virus were four or five times larger, very well marked, irregular, and crossed at the base by numerous bands, giving to them a reticulated appearance, and they were tender when touched. On one occasion only (except in the instance of a child vaccinated first with old virus unsuccessfully, and who only obtained slightly developed pustules from Passy lymph of the fifth generation), were the cicatrices sufficiently poor to approach in shape and aspect those produced by the old virus. Again, the scars produced by the old virus were never large and well marked enough to approach in character those left by the Passy virus, except perhaps, in the case of

the three children mentioned above. But in the case of all the other children M. Steinbrenner tells us that the difference between the scars was so decided, according to the virus used, that no one who did not know their origin would have taken them to be the result of one and the same affection.

These observations show the extent of deterioration which a virus, even in the hands of a good vaccinator, who would carefully select the pocks for propagation, undergoes in the course of nearly 2000 human generations. But the Passy lymph, by the time it reached M. Steinbrenner, had already been transmitted 240 times through the human system; and, if a progressive weakening of energy takes place, it was to be anticipated that it would have already, in the course of four years, have shown some loss of power. M. Steinbrenner's observations made with lymph from other new sources appear to show that this deterioration had really occurred to a slight extent. In 1841 he received from MM. Hering and Jaeger (of Stuttgart) some new virus, which had only passed through twelve human generations; and, in the same year, from Professor Salgues (of Dijon) lymph from another new source at Pellerey, which had only passed through fourteen human generations. Comparing the results produced by the latter with those produced by Stuttgart lymph of the seventeenth generation, he could discover no difference whatever; and the course pursued by the vaccine disease from both these last-named sources agreed generally with that pursued by the disease when developed from the Passy lymph, and the scars left in all the instances were equally good. Still, there were some differences in result, all of which were in favour of the younger sources. Thus, whereas engorgement of the axillary glands occurred in twelve out of sixty vaccinated with Passy lymph, this symptom was observed in twelve out of fourteen vaccinated with the Stuttgart virus. The complete desiccation of the

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vesicles from Stuttgart virus occurred also one day later than of those from Passy virus, namely, on the seventeenth instead of the sixteenth day, and the fall of the crusts took place from the twenty-fourth to the thirty-second day (mean twenty-seventh day), instead of the twenty-sixth day as a mean. Another point connected with the constitutional disturbance is to be noticed, namely, that in seven of those vaccinated with Stuttgart virus a second eruption, either all over the body or confined to the arms and chest, was observed about the twelfth or sixteenth day; it consisted of red-pointed vesicles, with a slight red areola; they disappeared in about twenty-four hours, leaving only little hard-crusted tubercles about the size of a pin's head. A similar eruption was only once observed after using the Passy virus, and never once after using the old lymph. Whatever the severity of the local or general symptoms produced by the Passy, Stuttgart, or Pellerey virus, no accident at any time resulted; and even when the fever was most intense it subsided in twenty-four hours or less.

In 1845 M. Steinbrenner had an opportunity of testing the change which had taken place in the Stuttgart virus after undergoing about 200 human transmissions, just as he had in the case of Passy virus three years before. Having met with a cow having cow-pox, the matter, however, being somewhat advanced, he vaccinated two children with it in sixteen places. Only one of the sixteen punctures took, and a fine pustule was produced, which ran through a regular course. From this he vaccinated two more, making again sixteen punctures in all, and every one succeeded. In carrying out the third and fourth generations he made a comparative experiment, inserting Stuttgart virus into the one arm and the new virus into the other, taking the lymph on the eighth day. He found the vesicles run parallel up to the thirteenth day, after which the suppurative period was rather shorter in the case of the

Stuttgard than of the vesicles from the new virus, and desiccation was complete by the sixteenth day, while it was not complete in the vesicles from the new virus until the seventeenth day.

Similarly to Bousquet, Steinbrenner endeavoured to measure the relative energy of the lymph from these sources by the successes attending punctures. Out of 200 punctures with the old virus he obtained only 164 vesicles, from 200 made with Passy lymph he obtained 186 pustules, and from 200 made with the younger Stuttgard lymph he obtained 192 pustules.

Steinbrenner's comparative experiments with Passy, Stuttgard, Pellerey, and his own new lymph, when taken into consideration with those made with Passy and the old virus, show how slowly, but at the same time how surely, the vaccine virus becomes enfeebled. Even after four years' successive transmissions the results of the Passy virus were very nearly the same as those obtained by Bousquet, and it acted much in the same way in 1841, after it had undergone some thirty-six additional transmissions at the hand of Steinbrenner.

But even all these observations do not determine the most important point for us to know. They tell us of lessened energy in the production of the local and general phenomena of the vaccine disease, but they do not tell us directly that the *antivariolic power of the vaccine is lessened at the same time*. Time alone and careful inquiry into the future history of persons vaccinated with younger lymph will show whether the vaccination they received is more stable in its protective power than when an older virus has been employed. Probability is all in favour of its being more protective, since the vaccine disease follows a course more closely similar to that of the exanthematous diseases with which we are familiar; there is more distinct vaccinal fever, sometimes a secondary eruption even is produced, the arcola is more

pronounced, and the duration of the disease is more prolonged. Besides, if it be true, as I think it has been proved to be, that the more perfect the local phenomena are the greater is the protection afforded, it can scarcely be doubted that an advantage, and a considerable advantage too, will be found to lie on the side of more recent virus; and if so, the protective power of the vaccine virus, whose product has, as a result of frequent human transmissions, become less perfect, must be held to be less than it was in its earlier generations.

In one sense the antivariolic power of old lymph is certainly reduced below what it was in earlier generations. I mean in the sense that it is less certain to develop itself in the system when the capacity of the system for allowing of the development is small. It succeeds less certainly and frequently in producing a pock, even when the vaccination is performed by the same hand upon an unprotected subject, than newer virus does. Adopting the hypothesis of Dr. Beale, it would appear to be possessed of less vitality, and the result of its development can only be a virus of equally low vitality with itself, capable of effecting an equally imperfect change in those elements in the blood which are the subject of its operation. Thus, the newer virus has been found to succeed when old virus has failed to produce any effect. In a case of very small capacity for development, related by Steinbrenner, in which the insertion of the old virus had failed, the new virus, eight days afterwards, succeeded, although, even then, only a feeble development took place. In another instance he had vaccinated a child with old virus three years successively without effect, but produced a perfect result on trying the newer virus, although the vesicles were not quite so fine as that lymph usually produced. Again, when the capacity for development has returned after a primary vaccination, the newer virus produces pocks more

readily and certainly than the older. I have already mentioned the experiments in revaccination performed by Bousquet and Boucher of Versailles. There are others to the same effect. In 1839 Bousquet revaccinated 131 persons, and thirty times obtained results with the new virus equal to those of a primary vaccination. Steinbrenner also, out of 142 revaccinations with the old virus, only got thirteen absolutely normal vaccine eruptions, that is, in one case out of eleven; whilst, operating with newer lymph, he obtained perfectly normal vesicles in twenty out of sixty-five subjects, or in one out of three and a quarter; the ages of the subjects and the period of primary vaccination being absolutely the same. Among these twenty there were four whose revaccination had been attempted unsuccessfully seven months previously. In twenty of the sixty-five he made comparative experiments with the new and old virus upon the two arms. Seven of them got normal pocks from the new virus, and only two these seven had any pocks at all from the old; five others out of the same batch had modified pocks from the new virus, and four out of the five modified pocks also from the old; in the remaining eight no result followed from the insertion of either the new or the old virus. It may be inferred from this that, had only the old virus been employed, in twelve of these twenty there would still have been left some capacity in the system for the development of small-pox on the contagion being received into the constitution.

All these facts, which I have detailed thus at length, indicate to my mind the extreme *desirableness of occasional renewal of the current lymph* from fresh sources. But they do not demonstrate its absolute necessity, nor yet the worthlessness of the vaccinations performed with the virus now commonly used in this and other European countries. Whence that virus is derived, who can tell? Some of it may be—I believe that employed at the Jennerian Insti-

tution is—the direct descendant of the virus originally obtained and used by Jenner or Woodville; but at various periods and in various places new sources have been opened, and the lymph from these has become inextricably mixed up with that formerly in use, so that it would be a hopeless task to unravel the tangled skein or to trace the genealogy of the lymph employed by medical men, except in a very small number of instances. The practical conclusion is that persons engaged in vaccination should look mainly to their results, and particularly to the sort of pock, the amount of vaccinal fever, the sort of areola, and the duration of vaccine disease, which follow habitually the insertion of the virus which they are generating in children week after week. If these correspond tolerably closely to those obtained by the newer viruses whose effects I have been detailing, good; if not, the sooner they search for a thoroughly normal vaccine, and change their supply, the better for their patients, and the better too for the reputation of the protective. But even this will not make up for carelessness. Still, after all, whatever the condition of our supply in this country may be, there is the reassuring thought that, at the worst, vaccination, as commonly performed now, is absolutely protective against ordinary smallpox contagion in the large majority of persons; and even in epidemic seasons it is only a minority who suffer from an attack.

Individual constitutional abnormalities, as influencing the occurrence of post-vaccinal smallpox.

But, apart from the various causes of imperfect protection we have been considering, we are almost driven to admit that the occurrence of post-vaccinal smallpox may sometimes depend upon certain *constitutional abnormalities*. I have shown that a previous attack of smallpox constitutes

a higher protection than passing through the vaccine disease. Now, cases are on record of second attacks of smallpox, and even third attacks, taking place at very short intervals from a primary attack. Bousquet* cites some very remarkable instances of this. Lamure has seen it occur twice in the course of one year in the same person; it was dangerous the first time, and fatal the second time. Bérard, in his 'Essay on the Anomalies of Smallpox,' states that his colleague, Professor Golfin, met with smallpox in a little girl three and a half years old, who had had it at eight months of age, under his own observation. M. Baudot, surgeon to the Prince de Condé, treated a little girl eight years old for smallpox, which was severe enough to leave scars, and yet in the next year she had a second attack, which was recognised to be smallpox by M. Jadelot, who was called into consultation. M. Laudun, at Lyons, in 1812, saw smallpox in a young girl who, on the testimony of her medical attendant, had passed through the disease the year before. This case, however, is defective, inasmuch as the medical man might have been mistaken. No scars are mentioned as having been left. Another case is related by Thomson of a lady who had smallpox in her youth, and afterwards became the mother of six children, all of whom were inoculated while she suckled them, and on each occasion she took smallpox from her child. Dr. Innhauser's instance, before mentioned (p. 102), is a very remarkable one in this respect, three attacks occurring, the second severely, thirty-one years after the first, and the third, less severely, two years after the second. It appears to show us that the capacity for full development of the virus was in this case progressive, like the reappearance of susceptibility after its destruction by vaccination. Dr. Mason Good† seems to think that this "constitutional predisposition," in

* Op. cit., p. 166.

† 'Study of Medicine,' Cooper's edition, vol. ii, p. 389.

some instances, runs through a family, and quotes an example given by Dr. Barnes, of Carlisle, in which five individuals, sons or daughters of the same parents, having caught smallpox naturally in the summer of 1818, from which most of them suffered smartly, caught it again in February, 1822, and had it also smartly, though not quite so severely as on the first attack. It is quite enough to show that in some persons receptivity to smallpox does recur quickly after an attack of the same disease, without troubling ourselves very much about the frequency of the phenomena. It is admittedly rare, and so also is smallpox after a good and satisfactory vaccination. Still, that it occurs at all is sufficient explanation of those cases sometimes met with, where smallpox has not only occurred after good vaccination, but within a very few years from the performance of the operation, and that with lymph which had lost but little of its primary energy, as in the instance of post-vaccinal smallpox placed on record by Willan. Another case, quite in point, is related by Dr. Harrison,* of Horncastle, where a child, six months after vaccination, received mild smallpox with a moderate eruption from inoculation, notwithstanding that several individuals who had been vaccinated with matter taken from her arm resisted the same trial.

But there is another aspect in which the occurrence of unusual receptivity may be viewed. I have already had occasion to point out that, at a certain *age* of life, when important changes take place in the system, there happens, at the same time and in association with them, a very remarkable proclivity to the reception and development of smallpox—a natural proclivity which the protective power of a primary infantile vaccination is in some persons unable to control. Is there anything to lead us to the belief that possibly other constitutional *changes, brought about naturally*

* Quoted by Dr. Thomson, *op. cit.*, p. 157.

or under the operation of what we term "disease," may similarly occasion, temporarily or permanently, a condition of the system under which it becomes again capable of receiving and developing the virus of smallpox, or promote the return of susceptibility which we have seen to occur as years run on after a primary vaccination? One can readily conceive of such changes taking place.

I am not quite sure that the influence of change of climate may not be adduced as one of them, although here there are other considerations which this is scarcely the place to discuss. But the fact is well known that, both in the instance of smallpox and of scarlatina, and, let me add, of cholera, persons newly arriving in localities where the contagion prevails are especially liable to suffer from it, and that the chances of immunity are less for them than for those who have remained free from attack for some time while residing in the midst of its influence. I know of a remarkable instance of this in the case of two young ladies who, although freely exposed to the contagion of scarlatina at home, failed to take the disease, but caught it some short time after in Paris from a source which could not be traced.* Probably, in many instances, the immunity of persons living in an infected district from the commencement of an epidemic outbreak is due to some gradual accommodation to

* A totally different explanation of such occurrences may be suggested, namely, that a virus received into the system may not operate at once, but remain dormant until some change has been effected in the system favorable to development, or until some condition retarding or counteracting the tendency of the virus to operate upon the elements of the blood has been removed. It may be something of this kind which happens in those curious and very rare instances in which the virus, introduced by vaccination, has been said to have lain dormant for weeks or months, even for one or two years; for we can hardly believe that such reported cases which may be found scattered up and down in the medical journals are altogether fabulous. When I come to speak of vaccino-syphilis I shall have to allude to this again.

the altered condition of circumstances which the system may undergo, allied to what, in the case of remittent and other pernicious fevers, we term "acclimatisation;" and that the greater readiness with which new comers take the disease may be due to a want of such gradual accommodation. It is probably by some process of this kind that epidemics of all kinds die out. But even though we may admit that a contagious influence of a certain strength may thus operate more readily upon persons newly arrived where it prevails, than upon those who are "acclimatised" from residing in the midst of it, there yet remain cases which it is difficult to explain away in this manner. Our coarse physiology is not omniscient, and there are probably processes and changes occurring in our system too delicate to be recognised, except by some of their secondary results. We have not yet discovered, as a matter of fact, what it is in our system which renders us liable to smallpox at all, to scarlet fever or to measles, nor yet what the alteration is, which an attack of either of these diseases brings about, that renders us, as a rule, insusceptible of a second contagion. And yet we know, as a truth, that *some* alteration does take place. We frame a hypothesis to account for it, but this is only the scaffolding by the aid of which we hope some day to be able to perfect our pathological edifice. It is a very old idea that the occurrence of certain diseases leaves the system so altered that it becomes, in spite of vaccination, prone to the reception and development of smallpox. Willan* quotes, for instance, Mr. Goldson as holding, in his day, the opinion that some eruptive and contagious diseases, as measles, scarlatina, chicken-pox, nettle-rash, whooping-cough, or dysentery, may, by altering the state of the skin, remove the security derived from cow-pox, and he adds (referring to certain cases of post-vaccinal smallpox), "I have found on inquiry that more than

* 'Diseases of London,' p. 67.

half the children above mentioned who took smallpox after vaccination, had been intermediately affected with the measles, chicken-pox, scarlet-fever, cynanche parotidæa, or whooping-cough, but that in the rest there had not been any intervening disease." Well, this does not go for much, for probably of any hundred children taken at hazard out of the streets half would be found, between infancy and three or four years of age, to have had one or other out of this list of infantile diseases. We want comparative facts, trustworthy clinical histories of individuals, to settle the truth or falsehood of such supposed relationships. I think our minds are sufficiently prepared to give due weight to any positive facts that may present themselves in support of an opinion in itself not repugnant to probability. And one curious narrative is given by Dr. Paget,* bearing upon this matter. "At Swavesby, a populous village in Cambridgeshire, typhoid fever was very prevalent and severe in 1850. In 1865, on the occurrence of a few cases of smallpox, the inhabitants, in alarm, young and old alike, sought for vaccination. Accordingly, Mr. Daniell, the resident surgeon, vaccinated 616, many of whom had been vaccinated on a former occasion or had had smallpox. Of course, in a large proportion of these Mr. Daniell's vaccinations were unsuccessful, but by no means in all such. Mr. Daniell noticed the remarkable fact that the cases of successful vaccination, in those who had been already vaccinated or had had smallpox, were chiefly or almost wholly among persons who had had the typhoid fever in 1850. One was a woman aged sixty, who had had smallpox twenty years before, and whose face was pitted with it. The vaccination proceeded with her quite normally. She had had typhoid fever severely in 1850. It seemed as if the typhoid fever had not only produced a permanent effect on the constitution, but removed

* 'Harveian Oration,' 1866, p. 45.

that inaptitude for vaccination which had been left by the first vaccination or by the smallpox." It is much to be regretted that this narration is not more precise. What we should have liked to know is, how many of the 616 revaccinations were successful, and on what evidence the occurrence of typhoid fever in these particular persons fifteen years before was substantiated. We should also be informed how many of those in whom the vaccination was not successful had also had typhoid fever. Still, notwithstanding its deficiencies, the narration is one which, in the view of the possibility of a variety of causes, of whose operation we know nothing, modifying the constitution of an individual, is worthy of being kept in mind as pointing out what may be a very profitable field of scientific investigation. Analogy may help us a little in gaining an insight into this matter. Syphilis is a disease the virus of which, the local manifestation being long ago over, and perhaps almost forgotten, may lie dormant in the system until something happens to disturb in some way its equilibrium, and then the constitutional operation manifests itself. Thus, as we shall see further on, the disturbance effected by the vaccine disease may produce a condition favorable to the development of constitutional syphilis. So also, as we shall see, may an attack of smallpox. We are not yet initiated into all the mysteries of nature.

Are there any means which can be adopted for ensuring the most complete protection that the vaccine disease is capable of imparting, or of remedying the defective protection arising in after-life, either from natural causes or in consequence of the imperfection of the primary vaccination?

It is unnecessary, after all I have said about the care which it is incumbent upon the practitioner to take in the selection of the vaccinifer and the pock, to dwell more upon this part

of the subject. He ought never to have it out of his mind that his object in vaccination is to produce a real and absolute disease, and no precaution should be regarded as too minute and trifling which is calculated to ensure the perfect attainment of this end. He should remember that the vaccine disease is one which is not natural to man, that it is planted upon a soil to which it is foreign, and that it requires constant watchfulness and judicious cultivation to restrain its inherent disposition to deterioration. The vaccinifer should be robust, not weakly and cachectic; the pock selected should be perfect in its character, and the lymph should be taken at a period prior to the appearance of the areola.* The use of all hastily developed pocks or of pocks not perfect in their character (for the stage or day from their first rising) should be religiously eschewed; and in all cases (except when the use of dry lymph is absolutely unavoidable) the vaccinations should be performed directly from arm to arm, and the delay of transference be rendered as brief as possible. Vaccine lymph is subject to oxidation, and probably more so in some conditions of the atmosphere than in others. Schonbein has shown that it readily decomposes the peroxide of hydrogen; and, like all other animal fluids, it is subject to ordinary decomposition, and to the generation within it of organized beings. Where a selection of time is possible, the operation is best performed at seasons and times when the heat, either of the weather or of the room, is not great.† Of course, the watchfulness over the progress of the pock is a matter

* This is a better rule than that which adopts the eighth day. The eighth day is convenient because "this day week" is sure to be recollected as that on which a child is to be brought for inspection, but it is not uncommon to find the areolæ on that day strongly developed.

† There can be no question that no system of medical education can be complete which does not provide for distinct special and practical instruction in the art of vaccination. Such instruction should in every state be rendered imperative upon candidates for a medical or surgical diploma.

for parents, and, with very young infants, all they have to do is to see that they do not injure them themselves. But with older children the itching sometimes induces scratching or rubbing, and so, unless protected in some way, the pock may be seriously damaged at a stage when the harm effected is important. I have said that when, notwithstanding all precautions that he can take, a practitioner finds that the results he is habitually obtaining from his vaccinations are poor, he should seek for lymph from some better source. But as I have also shown that under the best possible selection, and with the utmost care in cultivation, the vaccine virus loses energy as its human transmissions become more and more numerous, so it follows that, from time to time, it is desirable that new sources of primary supply should be sought for and opened.

Now, this involves two questions—the one is the frequency with which natural cow-pox is met with in the cow, and whether it is sufficient to answer the purpose of occasional renewal of supply; and the other is whether the object desired might be attained equally well by any other means.

1. *Recurrence to natural cow-pox as a source of lymph.*—As regards *natural cow-pox*, it cannot be said that it is an extremely rare disease; it has been often discovered in dairies since the time of Jenner up to within the last few years. Probably it would be found still more frequently if sought after systematically; but then, in this country, it is no person's business in particular to look for it, and dairymen and farmers are naturally jealous of any information of disease of any kind in their stock getting abroad, or to the ears of the public. Perhaps one reason why it is less frequently met with than it otherwise would be is the general vaccination of the people in infancy, so that farm servants are less likely to suffer from its contagion. It is a disease which, with

apparently some climatic variations, occurs in many parts of the world. Besides England, it has been met with in France, Germany, and other European countries, in the United States and South America, and, admitting máttáh as one of its varieties, in India and Assam. In this country it was first discovered by Jenner in Gloucestershire, where Estlin also obtained his supply, and where, and in Somersetshire, it is still to be found from time to time. Dr. Sanderson* reports that Mr. Hancock, of Wedmore (Axbridge), informed him that he sees cases of it communicated to milkers every spring without exception, and that in one experiment which he made with the lymph taken directly from the cow the local effects were very severe, and the primary vesicle surrounded by secondary ones. Mr. Leonard, of Dursley, informed him that it appears in the dairies in his neighbourhood about once every three years. Mr. Larke, of Rendcombe, has seen it near Cirencester; Mr. Ormond, of Westbury-on-Trym, near Clifton, took a new supply from the cow about eight years ago, and has been continuing to use it. Mr. J. J. Evans, of Bristol, obtained a new supply about four or five years ago. The first inoculation produced extensive inflammation of the arm, and larger vesicles than those of ordinary vaccination; but in subsequent transmissions the severity of the local phenomena became less. He has taken lymph from the cow successfully eight or ten times. Then, Woodville, it will be recollected, found the disease the year after Jenner's discovery was published, in a London dairy; Aitken found it in several English counties and at Cork; Ceely in Buckinghamshire; and the discoveries in France have been very numerous—some of them I have particularly mentioned. Now, I say that a disease thus frequently met with cannot be called extremely rare; at any rate, there ought to be no difficulty in the way of any person desirous of improving his supply.

* 'Sixth Report of Med. Officer of Privy Council,' p. 213.

The announcement of a new source should always be made by the discoverer; and if this were done, and a very trifling amount of trouble taken in the public benefit, much good to the protection of our population might be effected.

There is, however, a practical drawback, which I must refer to, and that is the rapid passage of the eruption on the cow's teat from the stage at which it is most energetic, so that any information forwarded to an observer is apt to be too late for practical usefulness. This energetic stage is said sometimes to be over in from twelve to twenty-four hours. This may be one cause of the difficulty which has been felt by some observers in transmitting the disease, when met with, directly from the cow to the human subject by inoculation.

The *artificial production of the disease in the cow* has been attempted in various ways, some of which I have already mentioned; by inoculation of the lymph from the human subject, or *retro-vaccination*; and by inoculation of smallpox virus or of horse-pox virus into the cow. The virus of horse-pox itself has also been employed, and it has been proposed to use the horse as a medium for regenerating human lymph by *retro-vaccination*. We have now, therefore, to discuss the practical applicability of these several suggestions, and also of the practice of *animal* vaccination, a supply of cow-pox being kept up by successive generations on the cow itself.

2. *Retro-vaccination*.—The vaccination of the cow with human lymph has met with varied success at the hands of different observers at various times. In the earlier periods of the history of vaccination success appears to have been frequently attained; but at a later period, when the question of the degeneracy of virus from the old sources began to be mooted, more difficulty was experienced. It was on account

of the failures of retro-vaccination that the Wirtemberg ordinance of 1818 upon the subject, to which I have before alluded, fell to the ground. M. Fiard met with a similar disappointment when using the current lymph, but succeeded on obtaining what was stated to be lymph from a new source from England. Others have also succeeded when the lymph employed has not lost too much of its energy in passing through many human generations. In this manner Bousquet effected sixteen times successively the vaccination of cows ; and Steinbrenner, who had never been successful with old lymph, succeeded four times out of six when employing virus less deteriorated. He states, however, that his belief is that all he imparted to the cow was a local eruption, and the general constitution of the animal remained unaffected ; no fever or diminution of the secretion of milk was observed, and none of the supernumerary vesicles which in natural cow-pox are found at the side of the principal pocks were seen in these instances. His conclusion is that, although using an energetic virus of the second or fourth human generation, he did not develop *true* cow-pox, in the sense, I presume he means, of a constitutional disease. Both he and Bousquet vaccinated from lymph taken from these pustules, but are of opinion that no advantage was gained from the proceeding. Steinbrenner took the lymph on the ninth day, and made a comparative experiment, inserting the regenerated cow-virus on the one arm and human lymph of the corresponding generation on the other arm. He says that the result was perfectly similar in the two cases. I do not myself see what else he could have looked for, when the human virus employed had only passed through three to five generations from the cow, and yet he seems to regard the absence of any difference as a reason for not regarding the disease imparted to the cow as true cow-pox. Another reason for this incredulity which he gives is to my mind equally unsatisfactory,

namely, that the cow-virus took again on the human subject as readily as the human virus. Now, the difficulty experienced in imparting the disease directly from the cow naturally affected to man has not been universally felt, and, in many instances, where it has been perceived, has been due in all probability to the late stage of the pock from which the virus has been derived. One thing, however, is clear, namely, that if the human lymph was not improved in character by a renewed passage through the system of the cow, it was not rendered *less* effective. Some years ago Mr. Badcock made a number of retro-vaccinations and tried the lymph upon a few children, but he informs me that he concluded that it was not improved by the process, but that it was rather the worse for the transfer. Baron had previously come to a similar conclusion. Mr. Ceely,* who also made experiments in retro-vaccination, failed to see any advantage arise from it. Nevertheless, ever since the year 1852, when the vaccinations made with lymph obtained from London altogether failed, a constant supply of lymph has been kept up in Venetia by the persistent practice of retro-vaccination introduced there by Goldoni. It is stated that up to the year 1863 vaccination with seventh or eighth day lymph from these cows had never yet failed.

3. *Animal vaccination*.—But the advantages or disadvantages of retro-vaccination have become of little consequence, now that it has been found that it is quite possible to keep up a stock of original cow-pox lymph by successive generations upon the heifer.† Shortly after the introduction of

* 'Transactions of the Provincial Med. and Surg. Association,' vol. viii, p. 354.

† For further information on this subject than is given here consult 'Étude sur la Vaccination Animale,' par Dr. Lanoix, Paris, 1866; or Palasciano, "De la Vaccination Animale," 'Gazette Médicale de Lyon,' Dec., 1864.

vaccination into Italy, Troja, of Naples, conceived the idea of taking the vaccine virus from the vaccinated cow for the purpose of human vaccination, and the practice was pursued by him and his successors during many years for the benefit of the upper classes of society. On the death of Troja, Galbiati continued it, and although in 1810 these vaccinations were proscribed, yet in the same year, we are told, several distinguished statesmen availed themselves of this method for their own children. Galbiati was followed by M. Negri, who has had to bear the brunt of official opposition similar to that offered to his predecessors. It is to M. Negri that we owe the practice of animal vaccination as it is understood now. Troja and Galbiati had both propagated on the heifers a vaccine disease implanted upon them originally from a human source. In fact, what they practised was a retro-vaccination, the result of which was perpetuated through a series of animals. At first M. Negri followed in their steps, but after a time ceased to propagate the virus thus obtained. During the last twenty-four years that he has presided over the animal vaccination in Naples he has three times obtained a new supply of virus from cases of natural cow-pox, on each occasion maintaining the supply by an uninterrupted succession of inoculations from animal to animal. At the present time, so thoroughly have all the prejudices upon the subject been cleared away, that M. Negri vaccinates from the heifer in the course of a year from 3000 to 4000 persons, a number nearly equal to the annual births that take place at Naples; and for several years past M. Bima, of the Italian army, has used the animal vaccine alone for his regimental vaccinations and for the pupils of the military colleges. In 1864 M. Lanoix, a young Parisian physician, visited Naples, and presented in a memoir to the French Academy the facts which he had collected upon the subject. On his return he brought with him a vaccinated calf, and, after introducing the

practice of animal vaccination into Lyons, established it in Paris under the auspices of M. Depaul. In May, 1866, M. Depaul announced to the Academy* that cow-pox had been discovered at Beaugency, and that it had not only been used as a new source for human transmissions, but also for the vaccination of a heifer. On visiting the place he found the crusts still adhering upon the latter animal. They were carefully removed, and, the attached surface being softened and scraped, the matter thus obtained was inoculated by M. Brechemier into a third heifer in six places. Only one of these took; but a characteristic pustule being obtained, a fourth heifer was vaccinated from it in eight places, all of which took well. From this animal a fifth was vaccinated, and brought to Paris, the Neapolitan virus was permitted to lapse, and the new virus alone used for subsequent animal vaccinations. M. Depaul has recently announced to the Academy of Medicine† the conclusions which he has derived from his own experiments. I may ask that it be noticed that he finds no difference in the appearances when the Beaugency virus was used, which it is certain had never passed through the human system, and those observed when the Neapolitan virus was employed. This is important, as confirming the fact that the latter as well as the former was truly derived by continuous animal generation from an original cow-pox source. The following are his conclusions:

“1. There is no difficulty in transmitting cow-pox by inoculation from heifer to heifer.

“2. We have inoculated forty-five heifers in succession, and always with the same success.

“3. The method by incision, first employed, has no

* ‘Bulletin de l’Académie Imp.,’ t. xxxi, p. 590.

† “Expériences faites à l’Académie Impériale de Médecine avec le Cow-pox ou Vaccin Animal depuis le 12 Avril jusqu’à la fin de Decembre le l’année 1866.” ‘Compte rendu,’ 1867.

advantage over that by puncture, which succeeds equally well whether performed with a vaccinating lancet or with a needle.

“4. None of the beasts we have experimented on have suffered from any accident legitimately attributable to the inoculation.

“5. Only some of them were attacked with diarrhœa, or had it already when delivered to us. This has appeared to us explicable by the change in their food and lodging.

“6. The Neapolitan cow-pox served for the inoculation of the first three heifers. For the last forty-two we have uniformly used the Beaugency cow-pox.

“7. These two sources of cow-pox have given results to our eye perfectly identical.

“8. The successive transplantations of the same cow-pox has not appeared to influence the development of the resulting pustules. Those which the last beast exhibited presented the same characters and the same dimensions as in the first experiment.

“9. We have established that the progress of the eruption upon the heifers was a little more rapid than that of the vaccine pustule in the human species.

“10. The elevation has almost always begun to appear in the course of the third day, and, as a rule, suppuration commenced in the course of the seventh or eighth day.

“11. The state of health of the heifers has had a marked influence upon the development of the eruption. Such of them as became ill have presented pustules less developed.

“12. The eruption has shown itself exclusively at the points of inoculation, and we have never been able to detect any pustular appearance on other parts of the skin or at the mucous orifices.

“13. General reaction has appeared to be absent or nearly

so. In some of the heifers only have we had occasion to note a little drooping and heat of skin.

“14. Our experience at the Academy demonstrates that it would be possible to organize and maintain at a moderate expense, especially in the great centres of population, a service of animal vaccination.

“15. Spontaneous cow-pox is not so difficult to meet with as is generally believed. Two occasions of this kind have presented themselves during the course of our experiments.

“16. The cow-pox which has served for the most of our experiments has an origin the authenticity of which has appeared to us incontestable.

“17. The number of inoculations which may be made being illimitable, the quantity of cow-pox matter which each heifer can furnish is considerable, and in all cases more than sufficient to meet the exigencies of the most extended service.

“18. So far as our experiments go, syphilis is not inoculable upon animals of the bovine species.

“19. In the vaccinations which we have performed, whenever the cow-pox matter has been taken at the period which is now known to be most appropriate, the successes have been pretty nearly constant, and in all instances at least as numerous as when lymph has been taken from the infant.

“20. On the contrary, whenever it has been taken at too late a period, that is to say, on or after the seventh day, failures have become more frequent, and the number of pustules less considerable compared with the number of punctures.

“21. The results obtained with the cow-pox from Naples have not been inferior to those furnished by the Beaugency cow-pox.

“22. It is not uncommon, when infants are inoculated with the cow-pox, to see the period of incubation prolonged,

and the eruption not manifest itself until between the fifth and the twelfth day.

“23. Sometimes on the same individual the pustules exhibit a very irregular progress, some of them being far advanced whilst others are scarcely beginning to appear.

“24. Our experiments show that the pustules obtained by inoculation of the animal vaccine have an advantage in point of size over those which follow upon the use of human vaccine.

“25. The inoculation of cow-pox produces a more evident general reaction in the system, especially at the period of suppuration, when the local inflammation is most intense.

“26. These symptoms have never assumed a serious character in any of the children that have been inoculated.

“27. As respects the number of pustules obtained, we have arrived at pretty nearly identical results whether we have used the cow-pox or vaccine from the infant.

“28. As a result of cow-pox inoculation, we have observed in a certain number of instances the appearance of two, three, or even four pustules about one single puncture.

“29. This phenomenon, which is not absolutely foreign to human vaccine, is observed, however, much less frequently as the result of its inoculation.

“30. Every method of inoculating cow-pox upon the human species has succeeded equally well (incision, puncture with the lancet or needle). The chief thing is to take the virus at the proper moment.

“31. Cow-pox, like vaccine from the infant, often fails when used after being preserved for some time dried or in tubes.

“32. Without binding ourselves to a definite opinion upon the matter, our experiments, not being as yet sufficiently

numerous, it seems to us that in this respect human vaccine has some advantage over cow-pox.

“33. We have always inoculated successfully cow-pox matter which has been preserved in tubes for one month.

“34. Satisfactory results, also, have followed the use of it when it has been sent into the provinces or abroad.

“35. Will the protective power of cow-pox prove more durable and more complete than that of vaccine from the infant which has already passed through several generations? This is a question the solution of which will not be possible for several years.

“36. The number of our revaccinations have been too small to permit us to draw a rigorous conclusion from the facts in our possession.

“37. With animal vaccination it will be possible, in seasons when an epidemic is raging at one time in districts more or less distant from each other, to send into the infected localities one or several inoculated heifers, which would supply all the cow-pox matter necessary for proceeding to effect vaccination and revaccination on a large scale.”

The results of animal vaccination, as now pursued in Paris, correspond in all important particulars with those obtained when a virus only removed a few generations from the cow is employed. In both cases there have been observed a remarkably deliberate evolution, great fulness and firmness of the pock, a comparatively late appearance of the areola, prolonged limpidity of the lymph, late incrustation, and great delay in the falling of the crust. General febrile phenomena are also more obvious, and engorgement of the lymphatic glands is a more frequent accompaniment of the local irritation. M. Lanoix further states that while formerly, in revaccination from arm to arm, the successes obtained were only 8 per cent. (I presume he must mean *perfect* successes), now, by the use of animal vaccination, 40 per cent. of suc-

cesses are obtained. With respect to the higher degree of protection which animal vaccination affords, he says—"For twenty years past epidemics of smallpox in Naples have never acquired any great severity; and it is an opinion held by the inhabitants, that persons vaccinated with animal virus are not now exposed to danger from smallpox like those who, in their infancy, were vaccinated from arm to arm. It is in the Neapolitan army that the epidemics have raged with most violence. Might not the reason for this be found in the fact that nearly all the soldiers, strangers to the city of Naples, had been vaccinated in their infancy only with human vaccine?"*

The practice of animal vaccination is now fully established as a national method in France. All the vaccinations performed by Government authority in Paris are, I believe, thus effected, and the virus distributed into the provinces is all derived from the heifer. It has been introduced into Vienna, Berlin, Brussels, and St. Petersburg, and in all of these cities it has taken root. It is difficult to account for its adoption in so many places except on the ground that some real advantage attaches to it. I confess that I should be glad to see animal vaccination adopted also in this country, as a part of our national arrangements for the prevention of smallpox. I believe that good would result from it; if no other good, there would be this, that practitioners who are dissatisfied with the virus they are using might have recourse to the vaccinated heifer at any time for the improvement of their supply. It would not necessitate or even render desirable the abandonment of arm-to-arm vaccination; but its adoption would tend to meet the prejudices of some persons, who decline now to avail themselves of vaccination on account of the dangers with which they believe it is surrounded.

One word more about the method of effecting animal vac-

* *Op. cit.*, p. 15.

ination, and I shall quit the subject. When in Paris last summer I had an opportunity of studying the procedure as carried on by M. Chambon on behalf of the Academy. Arrangements are made by which a succession of heifers or calves of about the age of five months is provided for. They are carefully stabled, and fed as nearly as possible upon the diet to which they are accustomed. A vaccinating table of convenient construction is provided. It is a strong wooden table, with a flap, which is cut out semicircularly in the middle, so as to correspond in shape roughly with that of the body and legs of the animal. The flap being let down, the calf is placed with its left side against it, and, being then securely fastened, the flap is raised horizontally, so that the animal lies with the right side uppermost. The operator then proceeds to shave, with a dry razor, the right side of the abdomen, commencing from the udder, and extending over a space of about ten inches long by six or eight broad. The calf which is the vaccinifer, from which the virus is to be taken, is also securely fastened down in a similar manner upon the floor, and the vaccine matter is obtained from the pock by forcible compression of its base with a pair of spring forceps. The result is the rupture of the pock and an abundant flow of a thickish fluid, which is taken upon the lancet or into capillary tubes for the purpose of preservation. The animal on the table is vaccinated upon the shaven surface by puncture in sixty or seventy places, and means are adopted to prevent subsequent injury by biting or licking. Pocks, which finally attain the size of large human vaccine pocks, speedily begin to rise, and are used for the vaccination of children from the fourth to the sixth day. After this day the vaccine they contain is found to be less active, but still sufficiently so for the vaccination of another calf, for which the pocks left unopened are, therefore, used on the seventh or eighth day. The method of obtaining the vaccine matter

just described is different from that followed by M. Negri in Naples. His practice is to slice off the pustule with a lancet, and then, taking it in his fingers, he scrapes the attached surface and expresses the fluid from it. When a calf is done with it goes to the butcher. ,

4. *Variolation of the cow as a source of lymph.*—I have already stated that Thielé of Bazan, Ceely, Badcock, and others, have succeeded in inoculating the cow with smallpox virus, and in producing the vaccine disease thereby upon that animal; and further, that inoculating the human subject from the resulting pustules, they obtained vesicles undistinguishable from those of ordinary vaccinia. When Thielé wrote his account of his success, he had already transmitted the lymph through seventy-five human generations, and in twenty-one instances he had tested its protective power by the subsequent inoculation of smallpox virus. Mr. Ceely also vaccinated several children with the lymph procured from his variolated cows. He draws especial attention to the fact that the transmission of the disease, thus originated, from the cow to the human subject was a matter of some difficulty. Out of twenty punctures made from his first cow only six vesicles were obtained, and they appeared tardily; he describes the vesicles as fine and “pearl-like.” Out of sixteen punctures, again, made from his second cow, only seven vesicles were obtained. The areolæ did not appear until from the tenth to the twelfth day. In this difficulty of transmission Mr. Ceely recognises phenomena similar to those observable in vaccinating from primary or natural cow-pox. Subsequent human generations were effected more readily; but “in some instances the difficulties were not completely overcome, even in the second removes.” In the subsequent removes a marked improvement was observed in the development of the vesicles, and the active manifestation of the primary and

secondary symptoms were not less apparent than in the use of natural lymph under corresponding circumstances. The following is his description of the progress of the vaccinia as observed in the fourteenth human generation:—"In the majority of instances, in propagating from arm to arm, distinct papulation was apparent on the second day; on the third it was not only visible, but elevated and well defined; on the fifth and sixth vesiculation was abundantly obvious, and lymph was often taken on those days; on the seventh vaccination was frequently performed, and points were often charged; on the eighth the vesicle commonly exhibited a bold, firm, and glistening aspect; between this period and the ninth day the areola generally commenced (but in young infants, with tense and sanguine skins, it appeared *early* on the eighth); by the tenth day the vesicle was commonly in its greatest beauty and highest brilliancy, glistening with the lustre of silver or pearl, having the translucency and appearance of crystal, or shining with a pale blue tint, occasionally of a dull white or cream colour, bold and elevated, with a narrow centre and broad margin, or flat and broad in the centre, with an acute margin, occasionally not raised above the level of the skin; on this and the eleventh day an extended and general vivid areola existed, with more or less tension and induration on the integuments. *At this time the lymph was frequently pellucid*, and often perfectly efficient. From the eleventh to the thirteenth day gradually increasing; in many individuals, both children and adults, sometimes the entire vesicle, at others only the central parts, reflected a blue or slate-coloured tint from the congested or ecchymosed subjacent adventitious structures, proportioned to the texture and degree of translucency yielded by its desiccating epidermis. On the thirteenth or fourteenth day, particularly on clear skins, moderately thick, the vesicles attained a considerable size, *measuring often in their longest diameters six*

and a half or seven lines, and acquired a light brown centre, from commencing desiccation, which was surrounded with an outer margin of dull white or pale dirty yellow, soft and flaccid, and still possessing fluid contents. During this period the areola, of a dull red or damask hue, would revive again and again, and even to the sixteenth or eighteenth day, the period to which complete desiccation was frequently protracted. The crust commonly partook of the form of the vesicle; it was often prominent and bold, varying in colour from that of chestnut to that of a tamarind stone. It fell generally about the twenty-third or twenty-fifth day, often later.

“The cicatrices were of variable depth and extent. When the vesicles remained unbroken on a thick sanguine skin, they were deep, but on a thin skin shallow; they were not always proportioned in width to that of the vesicle, the smallest cicatrix often succeeding the largest vesicle; but the later the crest fell, of course the deeper the cicatrix, which on these occasions was often beautifully striated. . . .

“When the lymph in the first remove produced normal vesicles, and as soon as it had passed readily from arm to arm, the constitutional symptoms, though mild, were most commonly well marked. In infants, restlessness, fretfulness, and inappetency about the fifth or sixth day, were very common, sometimes as late as the seventh day; very few escaped feverish symptoms on the ninth and tenth days; many had vomiting and diarrhœa. From childhood up to puberty the primary symptoms, from the fifth to the seventh day, were unequivocally visible and often complained of; fever, vomiting, delirium, and diarrhœa were not unfrequently witnessed at the commencement or during the progress of the secondary symptoms. In adults, of course, more complaint was made; headache, chilliness, anorexia, and sometimes thirst, on the fifth or sixth day, increased on the seventh day, with

axillary tenderness ; but on the ninth and tenth days much general febrile complaint, disinclination, and even inability to leave the bed. But in several instances amongst young children little or no complaint was made or indicated. All the members of the same family vaccinated from the same source might be differently affected. One, for instance, would not cease from pastime, occupation, or meals, while another, particularly if older, would be indisposed several days. Neither the number nor the magnitude of the vesicles seemed to determine the amount of the primary disturbance. If properly developed, small vesicles often gave rise (*sic*) to marked constitutional symptoms ; and the most splendid vesicles were often seen with trivial, sometimes scarcely appreciable, disturbance. The secondary symptoms were often as active with three or four as with six or eight vesicles. Acceleration of the pulse was frequently noticed when no other symptoms appeared.”*

I have quoted this description *in extenso* on account of its close correspondence with that given by Bousquet and Steindler of the vaccine disease, as it appeared to them in the earlier human generations from the natural cow-pox. The chief points of similarity to which I desire specially to direct attention are the tardy or rather, to use Dr. Stewart’s word, “deliberate” evolution of the primary eruption, the late period of the commencement of the areola, the size and prominence of the vesicle, the length of time that the lymph remained clear, the delay of the fall of the crust, and the greater prominence than is usually observed of the constitutional symptoms. All these are characteristic of natural cow-pox in its early human generations ; so that no doubt can be left, I think, upon any unprejudiced mind that Mr. Seely, *in variolating cows, actually did produce in them true cow-pox*,

* Seely, loc. cit., p. 414.

and that the results of using their lymph were equal to those obtained by Bousquet and Steinbrenner in the use of natural primary cow-pox lymph.

The results obtained by Mr. Badcock were very similar to those described by Ceely. In a letter upon the subject with which he has favoured me he writes thus :—"The vesicles from my lymph direct from the cow were almost invariably smaller than those produced by the ordinary lymph, and, contrary to what has been generally represented, the lymph produces less local irritation than older lymph. When it has passed through four or five children the vesicles produced are much finer, very firm, not yielding much lymph, but clear, until the twelfth day, the areola commencing on the eighth day, rather extensive on the tenth day, and if the vesicle is not disturbed the crust will remain on three weeks. The vesicle differs from that obtained from the lymph from the National Vaccine Establishment ; the latter is not so regular in development, it is more conical in shape, and contains a more watery lymph, and the areola is frequently extensive on the eighth day, which I have frequently noticed. The cicatrix from punctures with early lymph is larger than after the old lymph, but perhaps not quite so large as that described by Bousquet. . . . I have noticed a supernumerary vesicle from the use of my early lymph, and it occurred from some I sent to Mr. Marson of the Smallpox Hospital. I will give you his remarks :—"I have found your lymph certain in taking effect, and in one of the last cases I saw, in which it had been used, there were two supernumerary vesicles, one on the arm, and the other on the shoulder smaller than those at the place of irritation. I have observed a like occurrence before from active vaccine lymph.'" Mr. Badcock has distributed his lymph very extensively, and has received a large number of testimonies from medical gentlemen expressive of their complete satisfaction with the results they

obtained. Amongst these I find two statements* of the number of vesicles obtained from the introduction of the virus in four punctures. Thus, in fifty-three subjects vaccinated in 1843 with Mr. Badcock's lymph, there were obtained four vesicles in forty-seven, three vesicles in three, two vesicles in two, and one vesicle in one instance. Again, in seventy-eight subjects vaccinated in 1845, there were obtained four vesicles in seventy-one, one vesicle in two, two vesicles in two, and three vesicles in three instances. *The activity of the lymph, then, is quite equal to that of the early human generations from natural cow-pox.*

5. *Generation of cow-pox from the horse and retro-vaccination on the horse.*—I have already mentioned the success MM. Lafosse and Cayrel obtained in inserting the virus of horse-pox into the cow and into the human subject, with the result of producing in them a true vaccine disease. I have only to add that M. Auzias-Turrenne† maintains that, although in retro-vaccination performed upon the cow vaccine virus is not regenerated—does not reacquire its lost energy—nevertheless it *is* regenerated when passed through the system of the horse. He regards the horse, as Jenner did, as the original source of the disease in the cow. In order to effect this regeneration, however, he insists that the horse on which it is performed must be sufficiently young.

With all these various sources of active virus at our disposal, there can be no excuse whatever for persistence in the use of a deteriorated virus. Objections have been raised to the resort to *animal* vaccination, on the ground that other diseases which are peculiar to the ox and horse might possibly be communicated to the human subject vaccinated from

* 'Correspondence from Members of the Medical Profession, relative to recent supplies of Variolæ Vaccinæ or Modified Smallpox.'—Badcock.

† 'Bull. de l'Acad. Imp. de Méd.,' t. xxxi, p. 535.

them. But I cannot regard this difficulty in a serious light. There may be—and doubtless would be—prejudices to overcome in respect to having recourse to the latter animal; but there can be no objection that I can conceive of to recurrence to natural cow-pox or to the systematic production of artificial cow-pox by variolation after the manner of Ceely and Badcock, nor yet to the keeping up of a supply of cow-pox by successive generations upon the heifer of the natural disease. All that is requisite for the attainment of these objects is a proper organisation, and I cannot avoid thinking that it would be a direction in which the action of the government of a kingdom such as ours might be worthily engaged.

6. *Revaccination*.—I have said that there is reason to believe that, apart from all faults attaching to the vaccinator, or to the lymph which is used, there does occur, from natural causes, a tendency towards return of susceptibility to small-pox contagion, commencing, probably, not very long after primary infantile vaccination, increasing with the growth of the child, gaining intensity at or about puberty, and not lessening again except under the influence of advancing age, when about twenty-five years have passed over the head. From fifteen to twenty-five years of age is the period of life in which (after infancy is over) persons are most liable to take smallpox; it is the same with vaccinated and unvaccinated persons, although the degree of liability is not the same. This fact, established as fully as any fact well can be, seems to point to the adoption, about the time of early puberty, of some special means of counteraction. Hence it is that *revaccination* has been recommended; not because it is absolutely necessary for *all*, but because it is impossible to predicate who, among the multitude vaccinated in infancy, has reacquired receptivity for contagion. Those even who

have so far undergone a complete vaccine disease as to have good and satisfactory scars left upon their arms may, in some instances, be thus open to suffer from smallpox ; and hence, in view of the impossibility of discrimination, a repetition of the vaccination is recommended for all. My own habit is to advise it at the time when boys or girls, in the middle classes of society, leave school to enter upon business or the occupations of domestic life ; or to say that revaccination and the religious rite of confirmation are things which ought to go together.

There is abundant *evidence to prove the efficacy of revaccination as supplementary* to the primary protection. In any outbreak of smallpox, and especially under severe exposure to contagion, a certain proportion of vaccinated persons are, as we have seen, pretty certain to suffer, either from true smallpox or from varioloid. Experience has shown that revaccination operates by removing the renewed liability to the disease. Thus, Mr. Marson, in the paper* I have so often had occasion to refer to before, says—" In 1838 smallpox attacked the children in the Deaf and Dumb Asylum, and one or two a week for three or four weeks sickened with it, when Dr. Babington, physician to the asylum, requested me to revaccinate the whole of the inmates—about 260. I did so. Four days afterwards another child was attacked, who had received the infection of smallpox before he was revaccinated ; but from this date the disease was arrested in the establishment. Some months subsequently a servant, who had come fresh into the asylum, had smallpox ; but the disease on this occasion did not spread to the other inmates. For upwards of seventeen years of my connection with the Smallpox Hospital not one of the servants or nurses of the hospital has been attacked by smallpox, although vaccination has been the only protection of many of them ; but I have always revaccinated them on their first coming to live at the

* Loc. cit., p. 386.

hospital. On rebuilding the hospital, lately, a large number of workmen were employed for several months after the arrival of the patients. Most of these workmen consented to be revaccinated; two only were attacked by smallpox, but they were amongst the few who were *not* revaccinated." Angenstein* mentions that during ten years he revaccinated 10,682 prisoners at Cologne; and although smallpox was several times introduced by fresh prisoners, those who underwent the operation invariably escaped, and the disease only spread among the officers who were not revaccinated, and among the prisoners newly admitted. Dr. French informs Dr. Seaton that, at Dinapore, smallpox broke out among the men of the 49th Regiment of foot, but that he put a stop to it by extensive revaccination. Prussia was the first country which commenced the general practice of revaccination in its army. The practice was not commenced in the British army until as late as 1858. In 1831-3 epidemics of smallpox occurred, and it was found that persons with even good scars upon their arms were liable to attack. Prior to that time revaccination in the Prussian army had been confined to those whose cicatrices of primary vaccination were not satisfactory; but it appeared that, under this plan, smallpox had attacked from time to time, in various garrisons, soldiers who had been exempted from the operation on account of the good marks upon their arms; and, consequently, an order was issued in 1831 for the indiscriminate revaccination of all recruits. This is also the practice now in the British army. In 1832 a battalion of revaccinated soldiers went from Brandenburg to Erfurt, where smallpox was prevailing at the time; but not one of them took the disease.

In 1834, among the soldiers who had been revaccinated successfully, there happened 31 cases of varioloid and 2 of true smallpox. Among the soldiers

* 'Year-Book,' New Sydenham Society, 1863, p. 467.

who had not been revaccinated, there happened 586 cases of varioloid and smallpox, and 38 deaths.

In 1836, among the revaccinated soldiers, there happened 8 cases of varioloid and no case of smallpox. Among the soldiers who had not been revaccinated, there happened 122 cases of varioloid and smallpox, and 9 deaths.

In 1837, among the revaccinated soldiers, there happened 7 cases of varioloid only. Among the soldiers who had not been revaccinated, there happened 87 cases of varioloid and smallpox, and 3 deaths.

The protection afforded here by the repetition of the operation is manifest on comparison of the numbers. Again, it appears that revaccination was rendered compulsory on every soldier entering the Bavarian army in 1843.* Since this time *not a single case of true smallpox has occurred* among the troops, and only a small number of cases of varioloid, none of which, however, were fatal. Heim's experience in regard to the Wirtemberg army is of a similar character. During a period of five years, among 14,384 revaccinated troops, there happened but one case of varioloid disease, and this was in a man in whom the revaccination had had only a modified success. Nevertheless, smallpox had been imported no less than sixteen times into divers regiments, and had prevailed in several garrisoned towns, but it had always respected the troops. Again, out of 84,248 revaccinated persons in civil life in Wirtemberg, only two cases of varioloid disease occurred in the course of five years; whilst in the same time, out of a population of 363,298 souls, 1058 cases of smallpox had happened among persons who had only been subjected to a primary vaccination. This relates to towns and villages where smallpox was prevailing during these years.

There is a common *popular notion*, with respect to vaccination, that the operation ought *to be repeated once in every seven years*. I cannot tell how often the question has been put to me, whether such is not the limit of the vaccine protection. My invariable answer is that a successful revaccination

* "Report of the Bavarian Minister of War;" 'Papers,' &c., p. 170.

at puberty is all that is required to supplement the infantile vaccination. It is at this period that the proclivity to small-pox begins to manifest itself most obviously. After twenty-five or thirty years of age it becomes, even in those who have undergone only a primary vaccination, much less apparent. At puberty a sufficient time has elapsed for the capacity of development of the vaccine virus to have recovered itself to the extent of permitting a fair result to be obtained; and there appears to be evidence that no further protection is necessary. One proof of this lies in the great rarity of attacks of smallpox among revaccinated persons of any age. In 1836 Heim vaccinated, for the third time, thirty-two recruits, who bore upon their arms the scars both of a first and a second vaccination, the second having been performed at dates varying from two months to nine years before. In only one of these did he get any result at all, and in that one only a modified result.

Now, out of these facts there appears to me to proceed a further inference, and that is that a *modified result of revaccination is little less satisfactory than a perfect result*, either as a supplementary protection or as evidence of a sufficient protection, at the time the operation is performed, from the primary infantile vaccination. For, call to mind what has been pointed out before as to the small number of complete successes attained by revaccination—at the best they do not much exceed a third of the trials—yet, after revaccination, all may be regarded as permanently protected, whether the result be complete or imperfect. One can hardly conceive that in the latter case the imperfect vaccine disease produced can add much to the protection, although it may exhaust what little capacity for development has been generated; so that we are under the necessity of falling back to the inference we have drawn from other facts, that a primary vaccination, and that too sometimes of an imperfect character

(from defective original and natural capacity for development), does itself give a sufficient protection against the casual contagion of smallpox up to an advanced period of adult life.

But still, failures in revaccination and modified results are not always to be looked upon with complacency; for we must recollect that, when vaccination fails, it will in a proportion of such cases succeed if the attempt be made again. And we must also bear in mind the comparative revaccinations made by Bousquet, Steinbrenner, and Boucher, with old and new virus, which show that, where the former fails to develop, the latter may nevertheless succeed. Hence the importance of taking care that revaccination be performed with lymph which has not lost its pristine energy by too many human transmissions. Heim seems to have thought that more frequent successes were obtained when the lymph for revaccination of adults was taken from persons who had good vesicles from revaccination upon their arms, than when the lymph was taken from an infant. But the general opinion in the profession is distinctly opposed to the use of revaccination lymph at all. I cannot help thinking that there is a little prejudice in this, based upon the very commonly incomplete character of a secondary vaccine. I have, however, seen revaccination pocks as fine and as well developed as any primary pocks obtained from the use of lymph such as is currently employed; and I cannot myself see why, in such a case as this, the lymph taken at a sufficiently early date should not be availed of, when infant lymph is scarce, as it is sometimes in epidemic seasons. I know that in 1863 revaccination lymph was, as a matter of fact, largely made use of in the lack of primary lymph. But no one with a proper sense of the responsibilities of his profession would presume to vaccinate from a pock that did not present to the eye the perfect characters of the vaccine vesicle.

PART II.

“THE DANGERS OF VACCINATION FROM THE INTRODUCTION
OF OTHER DISEASES INTO THE ORGANISM, AND THE
EXTENT TO WHICH THE VALUE OF VACCINATION IS
REDUCED BY SUCH DANGERS.”

ALLEGED DANGERS OF VACCINATION.

IN criticising, as I am about to do, the allegation against vaccination, that it has proved the means of introducing into the organism other diseases besides the vaccine disease, and the evidence by which such allegations are supported, it is important to bear in mind the very varied *relationship which two diseases, existing together in the body may bear the one to the other, as regards their origin.* It is a subject so broad and complicated as to be unfit for discussion in such an essay as this, so that I shall content myself with enumerating some of the principal kinds of association, whether the two diseases appear simultaneously, whether the one succeeds the other, or whether they, so to speak, overlap one another in point of time, so that one is not completed before the other manifests itself. Thus—

1. The diseases* in question may have to each other some sort of pathological relationship. Of this nature is the association of convulsions or dropsy with Bright's disease of the kidney, the association of pulmonary congestion or pulmonary apoplexy with heart disease, or of tuberculous meningitis with consumption. The one may be the result or a sign or symptom of the presence of the other, or both may be of the same essential nature, and dependent upon the

* I use the word "disease" here in the popular sense.

same constitutional cause. 2. The secondary affection may be the result of damage done by the first disease and its accompanying structural disturbance; of this nature is paralysis of one side of the body, when left after an apoplectic seizure has taken place, and its primary symptoms faded away. 3. The relationship of the first to the second disease in point of time may be, that it has imparted to the organism a predisposition to suffer from other and different causes of morbid action, either by virtue of some change it has effected in the system generally, or by virtue of local conditions which accompany or follow it. Examples of this relationship are seen when diphtheria happens as a sequel to scarlet fever, measles, &c.; in boils after smallpox; in erysipelas or hospital gangrene, when they attack previously healthy sores, or follow upon surgical operations or the wound of the lancet in vaccination. 4. The two diseases may be perfectly independent the one of the other in their origin, arising from the operation of perfectly distinct causes. The cause of each, however, may be applied at the same time, as when a man contracts itch by contagion on the same day and on the same occasion that he catches a severe cold; or the causes of the two diseases may be applied at some interval of time the one from the other; we have already seen an illustration of this when smallpox breaks out spontaneously in a person who some days previously had undergone vaccination. It is important to remark here that, apart from any absolute disease, the condition of the system from natural causes may be such as to favour the occurrence of actual disease under the influence of various causes, or in itself may be sufficient to account for the appearance of actual disorder. I may illustrate this by the condition of the system associated with the natural process of dentition. I do not pretend to embrace all possible modes in which diseases may become associated, or in which one may appear as a sequel of another, within

these categories; all I wish to impress upon my reader now is that the mode of association or cause of sequence may be other than he is disposed at first to suspect.

Now, it has been asserted, with more or less of truth, that certain diseases have been observed to follow sometimes so closely upon the act of vaccination and the vaccine disease, as to leave an impression upon the mind that they have resulted directly from some contamination imparted by the operation itself. In persons not habituated to the study of disease such an inference is natural, and indeed pardonable; but it is unpardonable in a medical man whose education should teach him to reason philosophically, and to consult his own experiences of disease before committing himself to hasty conclusions.

The diseased conditions which have been believed to be capable of being imparted in vaccination, and to have been imparted so frequently as to constitute a real danger in the operation, are mainly smallpox itself, scrofulous affections, certain skin diseases, as eczema, and, lastly, syphilis.

1. *Is there any reason to believe that smallpox is ever imparted in the act of vaccination?*

We have already seen that smallpox may appear upon the body at periods varying from the day on which vaccination is performed up to the completion of the vaccine disease; and that this has been the result of the prior reception into the system of the casual contagion of the disorder. Except where smallpox virus is actually introduced with the lancet, I believe this affords the only explanation that is possible of the occurrence of smallpox after the act of vaccination. It is in this way I think that we must even explain the occurrence of smallpox in those cases where Messrs. Brown and Furnell* using native cow-pox (Mátáh) lymph of an early human

* 'Med. Times and Gazette,' 1866, vol. i, p. 46.

generation, and producing genuine vaccinia, at last found the punctures made upon some European children to be followed by an attack of smallpox. Had it been otherwise, these would not have been the only cases which the inoculation of the Mátáh lymph would have given rise to; yet, up to that time, and subsequently at the hands of Macpherson, it only produced the vaccine disease. The introduction of smallpox virus on the lancet in vaccination can only arise in two ways—namely, out of gross ignorance on the part of the vaccinator, who has mistaken one kind of pock for another, or from equally gross carelessness in employing a lancet contaminated somehow with variolous matter. And in such an event what happens is this—not the appearance of the vaccine disease locally and then an outbreak of general smallpox, but a local smallpox pustule as ordinarily obtained by inoculation of the virus, and then on the eighth or ninth day the secondary and grand eruption.

So long as the performance of vaccination is restricted to educated practitioners, I cannot see the slightest ground for fear that a mistake would be made in taking matter from a smallpox instead of a vaccine pustule. The two eruptions cannot well be mistaken the one for the other by a person accustomed to see both.

2. Is there any reason to believe that scrofulous affections or cutaneous diseases are ever communicated by means of vaccination?

In replying to this question we may freely admit that sometimes scrofulous manifestations do succeed the vaccine disease, and, moreover, that it is not a rare event for certain eruptions upon the skin, such as eczema or the “crusta lactea” of authors, to appear shortly after vaccination. But it does not follow from this that the lymph which has been used is, as mothers are very apt to fancy, the cause of the attack,

or that it has been derived from an unhealthy child. The quality of vaccine virus, as we shall see presently, is not regulated by any specific healthiness or unhealthiness of the child who furnishes it, in the sense that it partakes in any way of that child's particular constitution. It is the same from a scrofulous or ricketty infant as from a robust one; it is vaccine virus, weakened and deteriorated somewhat, perhaps, in its energy, but nothing more. Besides, if it were contaminated—if it did partake of the constitution of the vaccinifer—what then? Is scrofula a disease capable of being communicated by inoculation of a secretion from one subject to another? or are the skin diseases, so commonly laid to the charge of bad lymph, capable of a similar mode of transmission? Nothing of the sort—neither of them are contagious—neither of them inoculable—although the very secretions and diseased material itself be used to make the attempt with. These maladies arise in a very different manner. How scrofulous affections generally arise I have already had occasion to point out; and I have only to add that skin affections, such as eczema, strophulus, &c., are very common diseases in infants constitutionally predisposed to them, and especially during dentition, or when children are supplied with food of an improper character, or when cleanliness is neglected. Glandular swellings, also, about the neck are often associated, as a result of the local irritation, with eczema of the head and face if at all extensive. And these things happen just the same in unvaccinated children as in vaccinated children, and were observed just as often in the days when vaccination was unknown as they are now; and they originated then from just the same causes as they originate in now. The most that can be said about their relation to vaccination is that the disturbance of the system in the vaccine disease may sometimes, like any other constitutional disturbance, or like smallpox itself, promote or assist in the

manifestation of a scrofulous taint if pre-existent, or may assist other causes which alone have been insufficient for the appearance of an eruption upon the skin. I cannot express the relation borne by vaccination to such outbreaks of cutaneous disease better than it has been stated by Mr. Paget:*

—“When eczema, impetigo, &c., occur, as, in any sense, a consequence of vaccination, the explanation is to be found in the fact that vaccination produces a certain amount of feverishness, and is followed by a few days’ loss of strength, states which, though they are quite insignificant in moderately healthy children, are favorable to the evolution of any constitutional disease or blood-disease to which a sickly child may be liable. In children and adults alike it is certain that a tendency to the external manifestations of eczema and the other diseases above named may exist for many weeks or months, and yet not take effect till some accident disturbs the health, and weakens, as one may say, the power of retention or repression of the morbid tendency in the blood. There is, indeed, scarcely a blood-disease of which the evolution may not be thus determined or hastened by an accidental injury, or by a casual loss of health. When, therefore, eczema, or any such disease, so appears after vaccination that the one may with any reason be regarded as the consequence of the other, the vaccination may be considered to have done no more than any accidental injury would have done. Indeed, even among ignorant persons the blame of these diseases is not nearly so often laid on vaccination as it is on accidental blows, falls, frights, colds, surfeits, and other such things. Perhaps the most common expression of all is that a child “cannot get so much as the scratch of a pin but what it brings out an eruption,” or “is followed by an abscess.” Now, vaccination may do, though I believe it very rarely does, what these several accidents may do, namely,

* ‘Papers,’ &c., p. 139.

by disturbing for a time the general health it may give an opportunity for the external manifestation and complete evolution of some constitutional affection, which but for it might have remained rather longer latent." But so deeply rooted is the belief in some minds that, because these maladies sometimes succeed the vaccine disease, therefore they are caused by it directly and solely; that, rather than run a remote risk of what mostly is by no means a serious ailment, of what may occur in a predisposed child even if vaccination be not performed,* mothers will prefer to accept for their offspring the chances of smallpox and all its dangers.

I have already (p. 84) mentioned the occasional occurrence of erysipelas after the act of vaccination. Let me add, therefore, here, as the proper place for the remark, that there is no reason for believing that this disease can be conveyed from a person suffering from it through the medium of the vaccine lymph.†

3. *Is there any reason to believe that syphilis may be communicated in the act of vaccination; and, if so, to what extent do the facts in our possession relating to such commu-*

* Mr. Havers gives the following occurrence in point:—"On February 15th, 1845, I was requested to vaccinate the child of the housekeeper at No. 6, Carlton Gardens. Not having any vaccine matter by me, I put off the vaccination to the following Monday. On calling on that day I declined to vaccinate because I found the upper part of the child's arms and chest covered with eczema. Pneumonia came on, and the child died on the following Thursday. I considered this as a lucky escape, as, had I vaccinated the child, nothing would have convinced the friends that it had not died of disease produced by vaccination."

† See Lyman "On certain of the Accidents which may follow Vaccination," 'American Med. Times,' vol. iv, p. 135.

nication detract from the value of vaccination as a prophylactic means of general application?

POST-VACCINAL SYPHILIS.

I think I have stated sufficiently broadly the question which of late years has been a subject of so much controversy both within the medical profession and outside of it. It is one too serious to be sneered down, too important to be dismissed without receiving, at my hands at least, a full, and I hope impartial, discussion. Unlike many diseases which vaccination has been accused of giving rise to, syphilis is one which may be transmitted from one person to another by inoculation of its virus by the lancet, so that there is no flagrant improbability *primâ facie* in the assertion of those who maintain that syphilis has been communicated at the same time as the virus of vaccinia, when the skin has been punctured in the operation of vaccination. But admitting the fact that such an occurrence has been met with, we shall be by no means compelled to admit all the theories and explanations which have been constructed to account for it. Nor shall we be debarred by such admission from taking into our consideration the frequency or infrequency with which post-vaccinal syphilis has been observed, and the circumstances which investigation may have shown to have determined the syphilitic affection in any actual instances. Imagine for a moment that, in some individual on whom the vaccine disease has been inoculated, syphilis appears subsequently, either in its primary or in some one of its secondary forms; no one, I presume, would *at once* infer that the same lancet which, charged with lymph from a vaccine vesicle, had been used for introducing the virus, had *necessarily* been the medium of inserting at the same time the syphilitic virus. But suppose, further, that in any particular instance it were

shown to be an indubitable fact that the lancet employed in vaccinating, and not some accidental source of infection, had been the medium of introducing the syphilitic virus, surely it would not follow that the lymph with which the lancet was charged was necessarily contaminated before the vesicle was opened. The lancet itself might have been foul from some accidental cause. So it is clear that the fact of post-vaccinal syphilis, if proved, will not, of bare necessity, involve the serious charge that, in receiving vaccination at the hands of a medical practitioner, a child runs the risk of receiving at the same time the taint of syphilis from the same vesicle which is to him the fountain from which he draws his protection against smallpox.

I have said thus much in order to clear the ground a little. The whole subject of post-vaccinal syphilis is encumbered with difficulties, which are not lessened by the controversial heat with which it has been discussed on both sides. It will be my endeavour to estimate accurately the value of the facts adduced, and to draw such inferences as a strict logic appears to warrant. It is not the first time I have had occasion to make public my views upon the question. A few years ago I wrote anonymously* somewhat fully upon it, and although I can still maintain as just much of the criticism which at that time appeared to me to be fair and warrantable, facts which have since then been announced have led me to adopt an opinion which I did not feel justified in adopting at an earlier period.

Medical authorities, as I have already remarked, differ very greatly in the opinions they have arrived at upon this interesting subject. Perhaps, before I go further, it will be as

* 'Medical Times and Gazette,' March, 1862.

well to quote a few of them. Let us commence with our own countrymen.

In 1856, Mr. Simon, the medical officer of the then Board of Health, addressed a circular to members of the medical profession, both here and abroad, as well as to foreign governments and to the heads of departments in our own public service, inquiring, among other things, "Whether they had any reason to believe or suspect that lymph from a true Jennerian vesicle has ever been the vehicle of syphilitic infection?"—and to this he obtained a very large number of replies. Most of them—nearly all from British practitioners—were in the negative.

Mr. Acton says—"I unhesitatingly affirm that I have never witnessed a single case that will bear out the supposition;" but adds, "although I have for a long time been fully convinced that infection cannot be thus introduced into the system, I have never felt myself justified in directly vaccinating healthy children from the virus of vesicles obtained from syphilitic infants."

Dr. Addison.—"To both these propositions my own experience would return a negative."

Dr. Alderson.—"No; but as a Commissioner of the Vaccine Board, I am aware of a single instance, some years ago, of bad results following vaccination with a lancet ascertained to have been foul and accidentally used in ignorance."

Dr. Balfour.—"In no instance (in army vaccination during eight years) did any symptoms ever occur, or any appearance present itself, which could lead to the suspicion that the lymph had been the vehicle of syphilitic, scrofulous, or other constitutional infection."

Dr. Barlow.—"I have no certain proof that it is ever the vehicle of such infection, though I have suspected it in the case of syphilis. No careful medical practitioner would, I

think, ever knowingly vaccinate any one from a person in whom there existed any suspicion of syphilitic taint."

Dr. Beatty.—"I do not think such a thing possible."

Dr. Bright.—"I have never seen such a case."

Dr. Brinton.—"No. (A negative which, I am aware, almost implies that I believe constitutional syphilis *not* to be infectious.)"

Sir B. Brodie.—"I have no reason to believe that vaccination has ever been the means of introducing syphilis or scrofula, or any other constitutional disease into the system."

Dr. Geo. Burrows.—"I have no reason to believe or suspect that the true vaccine lymph has ever been a vehicle of syphilitic, scrofulous, or other constitutional infection to the vaccinated person; but I do fear and suspect that unintentional inoculation with some other matter than true vaccine lymph has occurred in the hands of legally qualified medical practitioners."

Mr. Geo. Busk.—"I have never witnessed any circumstance which could lead to such a belief or suspicion."

Mr. Ceely.—"Although I have heard of such events, I have never witnessed anything of the kind in my own practice of more than thirty-five years."

Sir James Clark.—"I see no reason to believe the occurrence of either circumstance."

Sir Philip Crampton.—"I have no reason to believe that lymph from a true Jennerian vesicle has ever been the vehicle of any other disease."

Mr. Theodore Davis.—"I have never seen (and my observation extends over some thousand cases) the slightest reason to suspect that any other disease has been communicated with vaccination."

Mr. Erichsen.—"I do not believe that any *constitutional* disease can be communicated through the medium of vaccine lymph."

Dr. Arthur Farr.—“ I have never met with an instance.”

Dr. Robert Ferguson.—I have never known such effects.”

Dr. Greenhill (giving the collective opinion of practitioners at Hastings).—“ Such a case had never been met with by any of the members present, but was not considered to be impossible.”

Mr. Thos. Hunt.—“ No such case has ever come under my own observation.”

Sir Wm. Jenner.—“ In the discharge of my duties as physician to University College Hospital and the Hospital for Sick Children, I must have had, during the last six years, more than thirteen thousand sick adults and children under observation, and in no case have I seen reason to believe or even suspect that any constitutional taint had been conveyed from one person to another by vaccination, or that any other disease had been unintentionally inoculated.”

Mr. Jones, of Chesterfield.—“ During the past six years I find I have vaccinated about 1062 children. The result of my experience leads me positively to state that in no instance have I seen a true Jennerian vesicle the vehicle of syphilitic, scrofulous, or other disease.”

Dr. P. M. Latham.—“ I have never met with the slightest proof of it; and the suggestion of its being possible amazes me.”

Sir Charles Locock.—“ I have not any reason to believe any such contingency.”

Mr. Lord.—“ No such cases have ever come under my personal observation.”

Mr. Marson.—“ I have no reason to believe or suspect that lymph from a true Jennerian vesicle has ever been a vehicle of syphilitic, scrofulous, or other constitutional infection to the vaccinated person. . . . I answer on this point from the experience of having vaccinated upwards of 40,000 persons.”

Dr. Noble, of Manchester.—“ Having had a very extensive

experience bearing upon this matter, I must reply very decidedly in the negative."

Dr. Robertson, of Manchester.—"I have no reason to believe or suspect such a thing."

Dr. Stokes, of Dublin.—"I never knew or heard of syphilis being thus communicated."

Mr. Tomkins.—"After an experience of twenty one years as a vaccinating surgeon, and having vaccinated upwards of 40,000 children, I have never known syphilitic, scrofulous, or other constitutional affection to be communicated by the operation."

Mr. B. Travers.—"My experience has not furnished me with reason to believe that the true Jennerian vesicle has ever been a vehicle of syphilitic, scrofulous, or other constitutional infection."

Sir Thos. Watson.—"I have never had any reason to believe or suspect that lymph from a true Jennerian vesicle has ever been a vehicle of syphilitic, scrofulous, or other constitutional infection."

Dr. West.—"None whatever."

Dr. C. J. B. Williams.—"I do not recollect an instance in my own experience in which such untoward results have followed vaccination, but I have heard of such, and I cannot but think that they were *not* the necessary consequences of vaccination, but of its indiscriminate and careless performance, as in taking the virus from diseased subjects."

Mr. Erasmus Wilson.—"No. I meet frequently with parents who believe an eruption under which their child suffers to have originated in vaccination; but the origin is in reality traceable to a different source."

This is, of course, only a selection from the replies given to Mr. Simon, but they are those given by some of the profession most extensively known, and whose experience, either in the general routine of practice, the special practice of vacci-

nation, or in syphilitic and cutaneous diseases, has been the largest. There are several more who give, like Dr. Gull, Sir John Forbes, Dr. Parker, &c, a simple "No" to the inquiry; others who, like Dr. Greenhow and Dr. Goodfellow, had not made up their minds; and there are a few who either speak decidedly in the affirmative, or who express themselves in a manner which indicates a leaning towards it. Thus—

Mr. Ackerley, of Liverpool.—"I have no doubt that syphilis has been communicated from a diseased to a healthy child by means of vaccination."

Dr. Barlow.—"I have no certain proof that it is ever the vehicle of such infection, though I have suspected it in the case of syphilis."

Dr. J. R. Bennett.—"I have suspected that syphilitic disease may be communicated by vaccination, and I would not willingly sanction vaccination from an unhealthy child, however perfect might be the Jennerian vesicle whence the lymph was derived."

Dr. Fred. Farre.—"I cannot say. I certainly would not vaccinate from a syphilitic child, and therefore I suppose I *suspect* the possibility of infection being thus communicated, though I know of no *facts* either for or against it."

Dr. Fleming, of Glasgow.—"I have a suspicion that I have once seen syphilitic disease communicated in this way, but it is a very difficult point to ascertain with perfect accuracy."

Mr. J. Hutchinson.—"I believe that I have seen four or five instances in which local syphilitic affections were induced by vaccination performed under ordinary circumstances, and by duly qualified men. In one or two of these the constitution suffered also, as seen by appearance of disease in several parts, but in others it was not perceptibly affected."

Dr. Laycock.—"I think this may be reasonably believed or suspected, and I think a duly educated practitioner may

be a negligent, careless, or thoughtless practitioner, and may inoculate unintentionally with some other disease. Our knowledge of morbid poisons is too imperfect to speak positively as to what may happen or what may be suspected 'reasonably' or not."

Dr. Lever.—"I must say *yes*. I have known syphilis communicated to a child by the hand of a supposed but legally educated medical practitioner."

Mr. Martin, of Bristol.—"I believe it is sometimes the vehicle of communicating the syphilitic taint to children previously perfectly free from it. . . . I have on many occasions been called to cases of eruption markedly syphilitic occurring after vaccination, and in which I have failed to detect the slightest taint in either parent or in the other children of the same marriage."

Mr. Mordey, of Sunderland.—"I have had one case where the lymph, sent me by a medical friend who is a public vaccinator, did produce a syphilitic taint."

Mr. Startin.—"This is a difficult question to answer satisfactorily, as the reply must rest upon what is to be regarded as a 'true Jennerian vesicle,' as this vesicle, in a subject suffering under constitutional or acquired syphilis or from porrigo, or even scabies, might be still 'a true Jennerian vesicle,' though not a pure one, and these maladies I have many times seen transferred from such a vesicle. I have also seen the same maladies inoculated by public vaccinators from unintentional vaccination."

Dr. Whitehead, of Manchester.—"I have seen several instances of the transference of the syphilitic taint through the medium of vaccination, the lymph having been taken from a true Jennerian vesicle, or presumed to be so at least, in a tainted infant." (Mr. Simon has a note to the effect that he failed to obtain further particulars on application to Dr. Whitehead. There are cases, however, which I pre-

sume Dr. W. refers to in his book, to which I shall allude presently).

These replies are very unsatisfactory in their character, as I shall have to point out as I proceed with my remarks, for it is quite possible that syphilitic constitutional phenomena may appear in an infant after vaccination from a perfectly healthy child. Only Mr. Hutchinson, it will be observed, says he has seen a local *and* constitutional result follow the supposed foul vaccination, and I shall show how important this is to the validity of any inference drawn from such cases.

Among Continental observers of distinction we find on the *negative* side the following :

M. Ricord, (of course) who amusingly replies, "Non, non."

M. Chomel.—"Je ne pense pas que la pustule vaccinale puisse contenir, outre le liquide qui lui est propre, le germe ou le principe générateur d'une autre maladie, comme la syphilis."

M. Roy gives the following qualified reply:—"Le virus vaccin pris avec soin, *non mélangé de sang*, ne donne jamais lieu à une autre maladie, syphilis, scrofule, teigne, &c."

M. Moreau.—"Quand on inocule de la lymphé vraiment vaccinale, on ne produit que la vaccine, quelque soit d'ailleurs l'état de santé ou de maladie du sujet qui la fournit. Pour produire la syphilis il faudrait inoculer du pus venant d'un chancre vénérien et non d'une pustule vaccinale."

M. Rayer.—"Dans une très longue pratique, je n'ai point observé d'exemple de syphilis transmise par la vaccination. Les cas très rares de transmission qu'on a cité ne me paraissent pas concluants."

M. Rostan.—"Je n'ai jamais vu que le virus vaccin emprunté à une pustule indubitablement vaccinale ait transmis soit syphilis, soit les scrofules, soit toute autre maladie."

Dr. Sigmund.—"The germ of a genuine syphilitic affec-

tion can never be communicated by a duly qualified vaccinator."

Dr. Stolz, of Strasburg.—"Je ne pense pas qu'il soit possible d'inoculer avec le virus vaccinal un autre virus tel que celui de la syphilis."

Dr. Stromeyer, of Hanover.—"I hold either to be impossible."

In the *affirmative* we find the following replies :

M. Alquier, of Montpellier.—"Il se pourrait donc que le virus vaccin emprunté à un sujet entaché d'affection syphilitique ou de telle autre affection spécifique communiquât en même temps deux lésions morbides. Cette conclusion est basée sur des principes qui me paraissent fondés, mais dont il ne m'a pas été donné de faire la vérification directe."

Dr. Bamberger, of Wurzburg.—"I am indeed convinced that *contagious* disease, syphilis for instance, is communicable with the lymph in vaccination ; nay, such a case has even happened a short time ago in a town but a few miles distant from this place."

M. Guersant.—"Oui ; je crois qu'on peut inoculer la syphilis par la vaccine ou toute autre maladie contagieuse."

Mr. Simon's own opinion at the time he wrote (1857) was that, in the cases then on record in which persons pretending to vaccinate did really *effect* a syphilitic inoculation, it is almost certain that the matter of syphilis was used by the vaccinator instead of vaccine lymph. He tells me, however, that since the publication of the account of the Rivalta outbreak, to which I shall allude hereafter, he has somewhat modified his opinion, and that he is disposed to believe that cases of reputed vaccino-syphilitic inoculation have arisen from the admixture of syphilitic matter with the vaccine in such a manner as might readily be comprehended to occur among careless and dirty people.

Now, what do all these replies from British and Continental observers indicate? Surely no more than this—that if syphilis ever is communicated by the act of vaccination, whether because the lymph is a vehicle for the transmission of the virus from the vaccinifer or because of the carelessness or ignorance of the vaccinator, it must be an event of extreme rarity. I believe I shall be able to prove satisfactorily, before I have done, that such a thing may happen, that such a thing has happened, but that this fact no more detracts from the value or general innocuousness of vaccination than the occurrence of post-vaccinal smallpox, or than a death from accidental hæmorrhage detracts from the general usefulness or the propriety of extracting a painful or carious tooth.

The present state of medical doctrine as respects syphilis and its mode of transmission.

In order to secure some sort of standing-place upon which to discuss this important question, and in order, at the same time, to render what will follow intelligible to the non-professional reader, it is necessary that I should commence by giving an outline of the present state of our knowledge in respect to *syphilis*, and the *mode of its transmission independently of vaccination*.

1. It is now all but universally admitted that there are met with in surgical practice two kinds of primary venereal sores in which certain marked distinctions are observable.

First, there is the *simple contagious ulcer* of the genitals, the “soft” or “chancroid” ulcer. This is a purely *local* disease. It is accompanied by *suppurating* enlargement of the lymphatic glands of the region in anatomical relation to the seat of the sore. It is *non-infecting*; that is as much as to say that it is not a disease of the whole system, but only of a part; the general constitution is not affected (or *infected*)

by the virus which produces it. It can be transferred or transplanted by contagion or by inoculation from one part of the body to another, and thus the person who is the subject of it may come to exhibit several sores of a similar character at more parts than one, or it may be inoculated from its first seat by an operation upon any other part that is desired. It is, therefore, said to be *auto-inoculable*. The general system not being infected, an attack of the primary disease is not succeeded at any future time by general symptoms; it is not followed by what are commonly called "secondary" symptoms. And the primary attack affords no immunity against a future contagion.

Secondly, there is the *true chancre*, the "indurated" or "Hunterian" chancre. This, although at first apparently a local disease, does not remain long a purely local disease. It is accompanied by indolent, *non-suppurating*, multiple enlargement of the related lymphatic glands. It is an *infecting* sore; that is to say, it shortly becomes accompanied by a general contamination of the whole system, which is said, therefore, to be "infected" by the virus. And this general infection of the system is of a character which renders it proof against a second invasion of the primary disease. In this it resembles then other virulent diseases, such as the vaccine disease itself, and smallpox, measles, scarlet fever, &c. (I am speaking now in general terms, without reference to the duration of the protection in any case). Hence the primary malady cannot be transplanted, either by contagion or inoculation, by an operator, to any other part of the body of the sufferer. It is *not auto-inoculable*, nor can true chancre be transplanted to the subject of it from any second person labouring under the same form of disease. And the general infection of the system, which is the cause of the protective power, exhibits itself sooner or later in various ways. One way in which it shows itself is by certain general symptoms; general, because

varying or more or less universal in the seat of local manifestation; their remarkable character is ubiquity. These manifestations are what are called *secondary* or *tertiary* symptoms. To this form of venereal affection alone ought the term "syphilis" to be confined. Whenever in the succeeding pages I use the term, it is this constitution-infecting form of disease which will be meant.

But we should be placing boundaries where Nature cannot be shown to have absolutely defined them, were I not to add that all this that I have said must be taken as the expression of a *general* rule, rather than of one which we can in the present state of our knowledge assert as absolutely universal. Thus, it happens that cases arise from time to time to puzzle the most acute and experienced observers. Such cases, which are probably explicable simply enough, but yet to us at this moment mysterious, are referred to by M. Ricord,* in a lecture delivered upon my present subject, as calculated to induce caution in our generalisations. Still, they ought not to be permitted to carry too much weight. At the most they are exceptional, and probably only apparently so on account of our own ignorance of all Nature's doings. At all events we can lose nothing by admitting thus much. And this additional modification of my statement must be allowed. Mr. Henry Leet† has pointed out that there is a stage of true chancre when it *is* auto-inoculable; but that is no real argument against the distinction I have drawn, but rather it is the exception which proves the rule. This stage is a very early one, before the protection derived from the general infection of the system has been obtained. Those who have followed me through the former part of this essay will find the analogue to this stage in that early period of the vaccine disease at which, the system not being fully affected, another

* 'Gazette des Hôpitaux,' January and February, 1862.

† 'Med. Chir. Trans.,' vol. xlii, p. 437.

vesicle may be raised, as in Mr. Bryce's experiments, by a second inoculation of the virus. Further, there is also, according to Mr. Lee's observations, a *condition* of the sore in which, at any stage of the disease, the indurated chancre becomes auto-inoculable, and that is a condition of irritation such as may be produced by blistering over the sore and the application of an irritating ointment. The secretion of true pus-globules by the sore is an essential element of this condition.*

2. The *general manifestations of syphilis* only appear after the expiration of some time from the occurrence of the primary disease. Syphilis, then, in its manifestations as a general or constitutional affection ("secondary" syphilis), has a period of *incubation*, short sometimes, but at other times prolonged. In this it will be recollected it resembles smallpox therefore, and also vaccinia. Ricord believes that syphilis may slumber in the system for as long as forty years. But this is very exceptional. Still, "constitutional symptoms rarely occur before the third week following the appearance of the primary symptoms, and more rarely still after the

* It will be observed that I have been speaking merely of *two kinds* of venereal sores, following different modes of progress. It does not follow, however, that they result from the operation of two distinct *kinds of venereal virus*. I may be permitted to quote Dr. Boeck's (of Christiania) views upon this matter. He says ('Recherches sur la Syphilis,' p. 68b)—"The soft chancre is produced by the same virus as the indurated chancre; the two forms arise from the *intensity* of their virus being different. Soft chancres are the product of the most energetic virus, which by its intensity develops in its circumference an inflammation which puts an obstacle in the way of absorption; indurated chancres are the product of a virus of less intensity, which does not develop an inflammation sufficiently strong to impede absorption. When the matter is very intense we get a definite soft chancre; if it is of little intensity we get a definite indurated chancre. It is with matter of an intensity different from these that we obtain intermediate forms of chancre, to which, with all our experience, we are uncertain whether or not the primary affection will be succeeded by constitutional syphilis."

sixth month" (Ricord). The interval is one marked by no characteristic disturbance of the general health. These general manifestations may break out, as we say, spontaneously, but truly in accordance with some etiological concurrence which our ignorance does not permit us to appreciate. But now and then there are appreciable circumstances which precede their outbreak, and these are ordinarily so similar that we cannot be wrong in attributing to them a causative operation. Perhaps the most universally recognised of these circumstances is the occurrence of deep disturbance, often very mysterious in its nature, of the *status quo* of the constitution. Bamberger, of Wurzburg, for instance, relates two cases where secondary manifestations did not appear until the occurrence of smallpox. This is quite in accordance with what we meet with in the case of other virulent diseases, in many of which we seem to recognise something more than the presence of the virus as essential to the development of the disease.

But in these instances of aroused latent syphilis what appears? *Not a chancre*. This never occurs except from the local application of the specific virus in a subject not previously infected. What we see arise in these cases is *some form of what we call "secondary" or constitutional syphilis*.

3. Constitutional syphilis is transmissible to offspring. No one now a days denies this. It is an *hereditary disease*, and, like the constitutional malady in the parent,* out of which it

* Dr. Charrier, confirming the opinion of Cullerier and Notta, concludes from his observations ('Arch. Générales, v^e sér., t. 20, p. 330), that the most important point is the presence of syphilis in the *mother*:—"1. Pour qu'un enfant naisse syphilitique ou qu'il le devienne pendant la première année de sa naissance, sans contamination directe, il faut que la mère soit ou ait été syphilitique. 2. Que le père syphilitique au moment de fécondation, à n'importe quelle période de la maladie, si la mère reste indemne de toute

springs, it exhibits its manifestations generally in the system. This is as much as to say that the symptoms of hereditary, or, as it is termed, "congenital syphilis," are what we call "secondary symptoms." *If we meet with a chancre in a child* we may be sure, then, that it arises, not from a hitherto latent systemic taint, but *from the local application of the syphilitic virus*. Rollet* has taken pains to show that in congenital syphilis the most ordinary seat of its manifestation is the *mouth* of the infant. It is not the only seat—anything but this—for the disease possesses ubiquity. The evidences of congenital syphilis may appear on a child at its birth; but, if they do not thus appear, it may be predicted with tolerable certainty that they will appear within a period far from indefinite. In fact, the period of incubation, or, to speak more correctly, of latency, is tolerably well ascertained. From the cases collected by Diday and others† it appears that the disease generally manifests itself before the completion of the first month of extra-uterine life. "When the third month is past there is no longer much probability that any symptoms of the kind will manifest themselves." Still, as an exceptional event, Diday has seen them deferred to as late a period as two years of age.

4. As to the transmission of syphilis by *contagion*. We are now entering upon controversial ground, and must be doubly careful where we rest our feet. No one questions the contagious nature of the primary sore; *but what of the local affections which arise as manifestations of constitutional syphilis?* Are these transmissible by contagion; or, rather,

syphilis, n'a pas d'enfant syphilitique. 3. Que par conséquent l'hérédité paternelle n'est rien moins que démontrée, et qu'il faut d'après les observations de MM. Cullerier, Notta, et les miennes, que la mère ait été en puissance de vérole pour que l'enfant naisse syphilitique." Boeck ('*Recherches sur la Syphilis*,' p. 508) seems to hold a similar opinion.

* '*Recherches cliniques et Experimentales sur la Syphilis*,' p. 242.

† Diday on '*Infantile Syphilis*' (Sydenham Society edition), p. 101.

is syphilis communicable from these manifestations to a person previously healthy, as it is from a primary sore? Hunter denied it; so, also, till a very recent period did Ricord, and so do still some of the staunch adherents to Ricord's school. But was Hunter, are Ricord's unflinching disciples, necessarily right? Rollet, Diday, and Henry Lee, no mean names in the list of distinguished syphilographers, may be mentioned as supporters of the contagiousness of secondary manifestations. Both of the former, in the works I have before referred to, dwell with emphasis upon the transmission of syphilis to the nurse from the mouth of the diseased suckling. To my own mind the proofs which have been adduced of this mode of syphilitic contagion are convincing, notwithstanding all the attempts which have been made to explain away such occurrences. M. Ricord himself, at last, was compelled to give way, and to give up his favorite dogma. In the lecture I have already mentioned, speaking of the contagion of secondary syphilitic lesions, he says—"Aujourd'hui je l'admets." But he qualifies his admission by explaining that he does not hold the doctrine in the same absolute manner as some of its partisans:—"Je viens aujourd'hui faire cette profession de foi; pour moi l'accident primitif reste la source constante, essentielle de la contagion; comme infinie exception à cette grande loi, il paraît que l'accident secondaire peut aussi se transmettre; des faits semblent le démontrer."*

And when I thus speak of secondary syphilis I particularly, but not solely, refer to the congenital disease with which, in the issue of this controversy, we have chiefly to do. Diday,† indeed, holds that congenital syphilis is especially virulent, "contagious by its slightest symptoms," and "only to be compared to itself." Henry Lee endorses this opinion.

* Ricord, loc. cit., p. 45.

† Op. cit., p. 129.

He says,* "The syphilitic poison appears to have acquired increased activity with the new life of the child, and, with that activity, to have become more readily communicable by contact." Both Rollet and Diday quote instances where, in a limited community, syphilis introduced by a syphilitic nurse-child has spread like an epidemic where it was unknown before.

Allowing, then, as I think we are bound to do, the contagiousness of certain "secondary" phenomena, what are the phenomena which appear on the recipient of the virus? All agree in this, that, whether the disease be really derived from a "primary" or from a "secondary" affection, *the disease which appears upon the recipient is always the primary form of syphilis*, always *a chancre*, or the induration which only requires the most trifling modification to become a sore. Rollet dwells most forcibly upon this; so does Lee, who maintains, in addition, that those secondary syphilitic affections which are not capable of being communicated under ordinary circumstances to another individual may, in like manner to the chancre, under increased activity become infectious. He explains in this way the difficulty which Hunter opposed to communication of the venereal virus to the inhabitants of the South Sea Islands, on the ground that it was almost impossible to carry a chancre so long a voyage. *A chancre appears at the place where the matter of contagion from the secondary lesion has been applied.* The imparted disease *never* appears *first* in the form of a constitutional malady, although at their due time secondary symptoms become manifested. *There intervenes an incubation of about three weeks.* Here, then, is at once a mark and *a test* by which we can distinguish between an imparted syphilis and one which is merely aroused from latency in an individual already tainted. In

* 'Med.-Chir. Trans.,' vol. xliii, p. 61.

the former case the first thing observed is a *chancre*; in the latter there occurs no chancre, but the disease appears off-hand in its *secondary form*.

5. As syphilis is contagious, so also it is *inoculable*, by the use of the fluids or the secretions of the diseased part introduced by a lancet into some part of the surface of the body of a healthy person, or applied to a blistered surface. This has been proved in respect of the primary chancre by actual experiment. The less said about the propriety of such proceedings the better, but still such is the case. Three such inoculations are recorded by Rollet—one performed by himself where the chancre appeared after an incubation* of eighteen days; another by Rinecker, where the incubation was twenty-three days; and one also by Gibert. But here, again, the controversy turns upon inoculability from secondary affections. The supporters of the affirmative doctrine adduce experiments performed by Wallace in 1835, by Vidal in 1849, by Waller in 1850, and by Rinecker in 1852. All these assert that the inoculations were successful, and the result was a *primary* lesion, having all the characters of the infecting chancre. A remarkable character of these inoculations was *the period of incubation* of the disease, which Rinecker tells us never appeared before the expiration of the second week, but *in general not till after the expiration of the fourth week*. There was thus, in these instances, a prolonged interval between the introduction of the virus and the appearance of the local disease, much longer than when inoculation is practised from a primary sore. The first alteration was always observed at the spot where the inoculation had been practised; the disease produced had a *progress essentially chronic*, so much so, indeed, that the local affection was still present when the general symptoms appeared,

* The term incubation here is used as the period between the insertion of the virus and the appearance of the *local* disease.

if not subjected to treatment. This is important for us to bear in mind. The general symptoms only appeared at the end of a month, and often much later, after the first manifestation of the local lesion. To all this Ricord objects in the lecture I have referred to. First, he complains that he himself, holding the contrary view, has been unfairly accused of having made the inoculations which convinced him of the noninoculability of secondary lesions upon already infected subjects, and asserts, on the contrary, that he has seen students free from syphilitic infection inoculated from secondary accidents, but always without result. Is it asserted that he did not wait long enough for the result? Then, M. Cullerier inoculated himself with the pus from syphilitic ecthyma, without result at any time. Next, he insinuates that the alleged successful inoculations of others were really made from primary, not from secondary, lesions, although apparently from the latter; that the "mucous patch" is the lesion always selected as the source of the virus, and that no distinction was drawn between such a lesion with ganglionic enlargement, which he regards as a primary affection, and such as are truly secondary. He further objects that of all secondary lesions the mucous patch approaches nearest to the character of the primary disease. But so long as the lesion from which the virus was taken was *constitutional*, and not the result of a local contagion, how can it matter that one form of the affection was preferred to another? Still, it may be, and probably is the case, that the virus cannot be obtained from every form of secondary lesion. But have all the successful inoculations been made from the mucous patch? Wallace's were not, Vidal's (quoted by Rollet) was not, and Waller's was not. In the last case the source of the virus was an infant with congenital syphilis. So far, then, as we can see at present, the argument is in favour of the affirmative proposition, and that

other lesions besides the mucous patch may furnish the virus.

6. Lastly, it has been asserted that syphilis may be transmitted *by inoculation of the blood of a syphilitic subject*. This is a step further. Can we take it? I beg particular attention to the evidence in this matter, because it is intimately associated with one mode in which it is believed that vaccination may produce syphilitic disease.

Viennois,* who, as we shall see in the sequel, has an hypothesis to support in connection with his view, labours hard to convince himself and his readers that such a thing is possible. How does he argue? First, from analogy, thus:—"Syphilis is a virulent malady;" this is his *minor* proposition. "The blood is contagious in all virulent maladies—in glanders, rabies, sheep-pox, smallpox, measles, plague, and diphtheria;" this is his *major*. *Ergo*, the blood is also contagious in syphilis. I do not object to arguments derived from analogy in their proper place, which is to show that there is no antecedent improbability in that which it is desired to prove. Still, I must say that it is not proved yet that Viennois' major proposition is absolutely true. What of vaccinia? Is not this a disease dependent for its origin upon a specific virus? Is it sufficient to inoculate with the blood to produce a vaccine vesicle? M. Rollet argues thus:—Is it possible to believe in hereditary transmission and the contagion of secondary lesions, and at the same time to deny the contagious property of the blood? "I ask such an one what is the system which puts the chancre, the primary source of the virus, in communication with the secondary accident to which this virus arrives at last—what, if not the vascular system, the blood?"† If the virus be contagious, then, he would have us conclude, so necessarily must be its vehicle. But is this a necessary con-

* 'Archives Générales,' June, 1860 p. 662.

† Rollet, *op. cit.*, p. 350.

clusion? Are we certain that the virus, contagious when received into the blood, and contagious when regenerated from the blood, is even virus at all in the intermediate stages? Do we know what it is, or what changes it may produce in the blood before the completely elaborated result appears in the secondary lesion? But both Rollet and Viennois take a higher position; and here we may follow them. They appeal to experiment—to one performed by Waller in 1850, one by Gibert in 1859, and to nine inoculations performed by a nameless individual, the results of which were communicated to the Society of Medicine of the Palatinate by its secretary.*

In Waller's case, which is narrated at length by Rollet,† the patient, from whom the blood was taken by cupping, is said to have had *primary* ulceration *five* or *six* times, but that "during the treatment of *the last two chancres*, which had occurred *at fourteen days' interval*, he began to emaciate and grow pale, and when the last chancre was cured tubercles formed upon the skin of the face, and spots all over the body." Now, I ask, is this the history of a case of infecting chancre? If not, the inoculation was worthless. If it were really a case of true syphilis, then I have another objection to raise, based upon the neglect to cleanse the surface thoroughly before the blood which was taken for inoculation came in contact with it. We have no guarantee in the history of the experiment that some of the virus from the local lesions was not carried away with the blood. The inoculation was performed carefully enough by scarification, and the application of charpie soaked in the blood on the left thigh of a child *suffering from lupus*. Thirty-four days after the inoculation two tubercles appeared at the seat of inoculation, but a similar tubercle appeared also where there

* 'Archives Générales,' 5me sér., t. xi, p. 603.

† Op. cit., p. 344.

had been no inoculation, namely, on the left shoulder. The tubercles on the thigh, having united, ulcerated ; but we hear nothing of multiple lymphatic glandular enlargement as a concomitant. Subsequently there appeared what was recognised as syphilitic roseola and papules and tubercles on the thigh and belly. Can any reliance be placed on such an instance as this? It is full of fallacies. Next there is Gibert's case. The blood was, in this instance, taken from a large squamous papule on the forehead. "The point of the lancet was thrust *into the circumference* of this papule, and charged with a blood *which was somewhat serous*,"* evidently then mixed with some of the diseased secretions within the papule itself. Surely no unprejudiced mind can place confidence in this as an inoculation performed with *blood only*. Lastly, there are the inoculations performed by the anonymous individual in the Palatinate. No details of these are given by either of the authors I have been quoting from ; and all we read in the communication of the secretary is the following : — "Of the nine inoculated with the blood, three were successful, and these were cases where a large absorbing surface had been rubbed." Can any statement be looser than this?

And if this were all that could be adduced in favour of the contagiousness of syphilitic blood when inoculated into the system, I, for one, should decline accepting the evidence as conclusive. But it is not all. An *inoculation against which no objection can be raised* on the score of neglecting any precaution against a fallacious result has been performed, and syphilis actually imparted. I take the case as it is quoted from the 'Imparziale' by Mr. Henry Lee.† Professor Pelpizzari, in 1860, inoculated two students of medicine with

* Rollet, op. cit., p. 347.

† 'Lectures on Syphilitic and Vaccino-syphilitic Inoculations,' 2nd edition, p. 198.

the blood of a patient affected with constitutional syphilis. The results of these experiments were negative. On the 6th of February, 1862, he again inoculated Drs. Bargioni, Rosi, and Passagli, with the blood of a patient named A. T—, aged 25, affected with constitutional syphilis, and who had not as yet been subjected to any specific treatment. The blood, in this case, was drawn with a new lancet from the cephalic vein. The patient was suffering from numerous confluent mucous papules on the left labium, towards the inferior commissure, corresponding to the point at which the primary lesion had appeared. There was in this situation a mucous tubercle developed upon the indurated cicatrix of a primary sore; or else the indurated primary sore had become transformed into a mucous tubercle. Mucous tubercles surrounded the anus. The inguinal glands were indurated and enlarged. A confluent syphilitic eruption existed upon the body, and there was enlargement of the posterior cervical glands. There were also pustules upon the head. The blood was taken from the patient's arm, at a part where there was no sign whatever of any eruption. The arm of the patient was washed, and the surgeon washed his own hands. The bandage and the vessel destined to receive the blood were new. As the blood was flowing from the cephalic vein some of it was received on some lint, and this was placed on the upper part of Dr. Bargioni's left arm, where the epidermis had previously been removed, and three transverse incisions made. This point corresponded with the insertion of the deltoid muscle. The same operation was performed on Drs. Rosi and Passagli; but in the case of Dr. Rosi the blood was already cold when it was applied, and in the case of Dr. Passagli the blood had coagulated. After the lapse of twenty-four hours, upon removing the dressing, nothing was observed at the seat of the inoculation in Dr. Bargioni's arm except a slight crust formed by the effused blood at the seat

of puncture. At the same time the dressing was removed from the arms of the other two physicians, and nothing was seen worthy of observation. Four days afterwards every trace of the different inoculations had disappeared. On the morning of the 3rd of March Dr. Bargioni announced to Professor Pelpizzari that in the centre of the inoculated surface he had noticed a trifling elevation, which produced a little itching. Professor Pelpizzari examined the arm, and found at the point indicated a small papule, of a roundish form, and of a dull red colour. There was then no induration at the base of the papule, nor any enlargement of the corresponding axillary glands. To prevent its being rubbed, it was covered with some dry charpie and diachylon. Professor Pelpizzari examined it daily. On the eighth day the papule had augmented to the size of a twenty-centime piece. On the eleventh day it was covered with a very thin adherent scale, resembling silver paper, which, upon two succeeding days, became denser and less adherent, and in its central part commenced to crack. On the fourteenth day two axillary glands became enlarged to the size of nuts, and were movable and indolent. The papule remained indolent, but its sensibility was slightly increased. On the 19th pressure upon the crust caused a small amount of seropurulent matter to exude from beneath its edges, the pressure giving a little pain. The axillary glands had now become larger and harder, but continued indolent. There was no induration apparent at the base of the papule. On the 21st the scale was transformed into a true crust, which had commenced to be detached at its edges, and the part beneath was ulcerating. Slight induration now appeared at the base. On the 22nd the crust was detached, and a funnel-shaped ulcer presented itself, with elastic and resistant borders, forming an annular induration. These edges were swollen, adherent, and obliquely inclined towards the base of the

ulcer, which was covered with a very small amount of secretion. The pain was trifling. Dry charpie only was applied. On the 26th the ulcer had extended itself to the size of a fifty-centime piece. It secreted more, and the surrounding induration was considerably increased. Up to the 4th of April this ulcer remained stationary, but at that date its base appeared to be granulating. The corresponding glands remained swollen, hard, and indolent. There appeared at this date trifling nocturnal pain in the head, and the posterior cervical glands became somewhat enlarged. On April 12th there appeared on the surface of the body, particularly upon the sides of the chest and in the hypochondriac regions, spots of irregular form, and of rose colour, unattended by any inconvenience to the patient. The glandular swellings of the neck were well marked. This eruption extended itself, and became more confluent during the succeeding days. No constitutional disturbance, heat of skin, nor pruritus, accompanied this eruption, which went on increasing for eight days. On the 20th the cervical glands had increased in size and were harder. The chancre maintained its specific character, and exhibited no tendency to cicatrization. On the 22nd the colour of the eruption was decidedly coppery. Small lenticular papules were now perceived to be mixed with the erythema. The edges of the chancre had begun to granulate."

I have transcribed this case *in extenso*, since it furnishes a most important basis on which will be placed some facts explanatory of one mode, at least, in which vaccine lymph may be believed to be capable of becoming the vehicle of syphilitic disease.

Let me now recapitulate. Upon what facts, as regards syphilis, do we take our stand? 1. That syphilis is a disease which, after its primary manifestation, the indurated chancre,

has lasted a certain time, infects the entire system, and renders it (as smallpox and vaccinia do) incapable of receiving or developing a second contagion. 2. That, after a time, the constitutional infection manifests itself in certain secondary symptoms.* 3. That the infection may not thus manifest itself until some disturbance of the *status quo* of the constitution is somehow brought about. 4. That constitutional syphilis is an hereditary disease; but that when it appears thus, as an hereditary disease in the offspring of syphilitic parents, it exhibits itself, not in the primary form of a chancre, but in some of its secondary or constitutional forms. 5. That syphilis is, in its constitutional manifestations as well as in its primary, transmissible to healthy persons by contagion: and that, 6, when it is transmitted thus to the healthy, it always appears *first* in its primary form. 7. That when the disease occurs *first* in one of its constitutional manifestations it is a proof that it was not imparted by contagion, but is the result of an inherent constitutional taint. 8. That syphilis may be inoculated into a healthy person with virus taken either from a primary or from certain of the secondary forms of the disease; but that when inoculated from a person suffering under the constitutional forms of the disease the incubation is longer, and the primary disease which results is more chronic in its progress than when the virus is taken from the primary sore. 9. That syphilis may be communicated by inoculation of the blood of a person whose constitution is infected with syphilis; and that, however the

* The grand test of the true syphilitic nature of a sore is the subsequent occurrence of constitutional manifestations. Some reputed instances of vaccinal infection are wanting in this evidence. I myself once saw such an instance where there was a sore very much like a syphilitic one following vaccination, but it was shown not to be so by the absence of glandular engorgement and of secondary manifestations. M. Blot, in the discussion at the French Academy, related a similar case which was observed by Cullerier.

disease be inoculated, and from whatever source, the disease which *first* results is a chancre, the primary form of syphilis.

From this vantage-ground we may see fairly the position taken up by the contending parties—by those who hold the communicability and by those who hold the non-communicability of syphilis by the operation of vaccination—and perhaps observe and estimate the value of each movement more accurately than the parties themselves actually engaged in the conflict.

VACCINO-SYPHILITIC INOCULATION.

Let me put a case. It is not a hypothetical one. An infant who had formerly presented marked evidences of congenital syphilis, which disappeared under treatment, was vaccinated. Never were finer pocks seen; they ran through their normal course, and at their former seat left beautiful cicatrices. On the fourth day from the vaccination, however, a well-marked syphilitic exanthem became developed, with mucous patches about the anus. The parents laid the blame on the vaccine.

Another:—Several children were vaccinated the same day by the same operator, and with the same lymph. All did well with the exception of one, and here on the sixth day numerous large physacious pustules appeared, first upon the arms and subsequently upon the face and rest of the body, leaving characteristic ulcerations; the syphilitic nature of these phenomena was recognised, and the patient recovered under appropriate treatment. The parents did not fail to lay the blame on the vaccine.

But was the vaccine to blame?

Let us apply our *test*. A syphilitic affection imparted by inoculation appears as a *primary* disease. In these instances the phenomena which appeared consecutively to the vaccination were "constitutional"—they were instances, then, of latent syphilis, roused into activity by the development of the vaccine. The parents were to blame, not the vaccine on which they were too happy to shift their fault. And this is the history of the large mass of cases of alleged syphilitic contamination by means of the vaccine virus. Most of Dr. Whitehead's cases* are of this character.

Smallpox will operate in the same way. Here is a case related by Bamberger of Wurzburg.† A woman, six months gone in the family way, entered the hospital at Wurzburg with smallpox; the eruption covered the whole body, and was not modified "when the period of dessication arrived, certain pustules on the forehead, neck, nucha, and groin, took on the following characters. They became broad and flattened, and at the base an irregular vegetation was developed, moist at first, and surrounded by a bordering of suppuration which afterwards dried up. By degrees they assumed the character of broad, moist, mucous patches. The genital organs were examined at the time when these transformations were first noticed, and several old mucous patches were noticed, some cicatrices at the entrance of the vagina, from which an abundant discharge was taking place, and the glands in the neck were engorged." In this and in another similar case related by the same writer we see that the very pustules of the smallpox themselves underwent the syphilitic transformation, but it was not a primary, but a secondary disease that was developed. There was no vaccination here to blame. Let us learn clearly to distinguish such cases, and let parents

* 'Third Report of the Clinical Hospital of Manchester,' 1889, by James Whitehead, M.D., p. 92.

† 'Gazette Hebdomadaire,' 1858, p. 390.

know that in future the less fuss they make about similar accidents the better for their own reputation.

The elimination of a host of cases of this nature reduces the controversy within narrow limits. The question comes to be—1. Whether, in the performance of vaccination, where the lymph has been taken from a subject infected with syphilis, the seat of the puncture has ever become that of a *primary* syphilitic affection? and if so, 2. Whether this affection has been clearly proved, in any instance, to have been due to the inoculation of the syphilitic virus in association with the vaccine, *both having been furnished together from the true vaccine pock.*

Now we can dispose of *the first of these questions* readily enough. In the face of numerous cases now on record, there can in future remain no doubt at all of the fact. It is admitted on all sides.

But how came the virus at the seat of puncture? Now comes the “tug of war.” M. Viennois, and M. Rollet, who endorses and repeats his statements, together with others of their way of thinking, assert that, in certain cases (which they relate), the syphilitic virus was furnished together with the vaccine virus from the same vaccine pock, a pock normal in its appearance and course, and wholly undistinguishable from any other normal vaccine pock. They further assert that the virus arrived at the pock through the medium of the vascular system.

On the other side it is contended that any such contamination of the vaccine virus is an *impossibility*: and it is maintained that, whenever such an occurrence has apparently taken place, it has arisen out of a conveyance of the virus accidentally from some other source to the seat of puncture.

Which is right? The true answer lies at the bottom of a sea of difficulties, and is, moreover, obscured by a mass of heterogeneous concretions. Still we must endeavour to dis-

cover it, and to bring it to view as nearly as possible in its simplicity.

Primâ facie, Viennois and his adherents have the best of it. But *what have they to show in order to establish their proposition?* 1st. That in any given instance the lymph, stated to have conveyed the syphilitic virus, was taken from a subject infected with constitutional syphilis; but not presenting at the time any local lesion, the products of which were likely to have come into contact with the vaccine pock. This is essential, for those who admit the contagiousness of congenital syphilis say it is endowed with pre-eminent activity. Hence we must be satisfied that no syphilitic virus could have been conveyed to the pock except through the general vascular system. 2nd. That the parents and nurse of the vaccinifer, at least, were free from any local manifestation of syphilis. Nurses and parents will handle and touch vaccine pocks. We do not think it would be fair to ask more of the observers than can be expected to be ascertained.* 3rd. That the fingers of the operator were scrupulously clean, he not having been, with his knowledge, in contact with the products of any syphilitic affection. 4th. That the lancet used was free from contamination, not having been employed for any other purpose than vaccination. And, with this, we should be assured that no old rags, or anything else likely to be unclean, had come into contact with the recent punctures. 5th. That the parents or nurse of the vaccinated child were

* A case related by Glatter appears to be one in which the vaccinifer might have conveyed infection from such a source. A midwife, who had a sore upon her arm contracted from a syphilitic woman, nursed her grandchild, and carried him about in her arms. The child was vaccinated, and from him most other children in the place. In these the pustules passed into spreading ulcers, and affections of the mouth and condylomata about the anus occurred. Mothers of the vaccinated children got fissures on their nipples, and the affection even reached the husbands. (New Sydenham Society, 'Year Book,' 1863, p. 468.)

not themselves suffering at the time from any form of syphilitic affection, the products of which could by the inevitable handling or otherwise have come into contact with the punctures; and that the observer has made reasonable inquiries as to the likelihood of contagion from any other person. 6th. That, considering the difference in incubation between vaccinia and true syphilis introduced by inoculation from a secondary lesion, the law of nature has been followed—that is, that the cow-pox ran in the vaccinated child its regular course, and that, at its termination, the former seat of the pock became the seat of the true “primary” disease. We have a right further to demand evidence that there followed, after the expiration of a reasonable time, unquestionable manifestations of constitutional syphilis, at all events where the primary disease had not been early overcome by appropriate specific treatment.

The supply of these requirements would, I think, meet all reasonable doubts. But, before looking at the cases on record, to see if these requirements are complied with, we will discuss the theoretical objections of those writers who maintain the negative in the controversy, based upon the impossibility of the syphilitic contamination of the contents of the vaccine vesicle.

When we begin to talk of *impossibility*, let us not be oblivious of Dr. Laycock’s caution, in his reply to Mr. Simon’s inquiry, namely, that our knowledge of morbid poisons is too imperfect for us to speak positively of what *may* happen; and thus presume upon our ignorance, as many of Mr. Simon’s correspondents did. One of these designated the idea of vaccinal contamination as “simply absurd.” Steinbrenner* also writes thus:—“Il serait tout aussi absurde de croire qu’en inoculant la lymphé vaccinale prise d’un syphilitique, on donnait la syphilis à l’inoculé, qu’il serait

* Op. cit., p. 828.

absurde de prétendre qu'en inoculant le pus d'un chancre d'un individu qui a en ce moment de belles pustules vaccinales, on pourrait donner la vaccine à l'individu inoculé." But why is the idea absurd, and why is it impossible that the vaccine lymph taken from a vesicle in a syphilitic subject should be so far contaminated with syphilitic virus as to be capable of conveying the disease? Let us keep in mind that there is no question here of *quantity* of virus, but merely of its presence in the lymph taken from the vesicle at all. *First*, because it is not in accordance with M. Ricord's doctrine, that the primary disease can only arise from the virus furnished by a primary sore. This reason, on grounds I have specified, we may at once set on one side. The doctrine itself is untenable. *Secondly*, an argument is drawn from the experiments of Sigmund and Freidinger, who in numerous cases inoculated with a mixture of matter from a chancre and vaccine lymph, and never with any result than the production of a chancre; from which they conclude that the matter of chancre destroys the property of the vaccine, rendering it inoperative. But only a certain number of the experiments were made upon persons who alone were fair subjects for it, namely, those who never before had the vaccine disease or smallpox; and even with these it could not fairly be said that the vaccine virus was destroyed, unless it had been proved by a second inoculation with the vaccine lymph alone that these persons were not among the few who resist the disease, or that the vaccine lymph used was not itself in fault. This is a fallacy, and to my mind a damning one, which attaches to the experiments of these observers. Sperino, of Turin, made seven similar experiments, but only one of them was on a person who was unprotected by previous vaccination or smallpox. The result, in this instance, was the very converse of the results obtained by the German physicians. Both diseases were actually imparted at the punc-

tures. The vaccine took the lead, as from its shorter period of incubation might have been anticipated; and on the sixth day the vesicles of vaccine were finely and characteristically formed. As the stage of desiccation came on, true venereal chancres appeared. In three of the cases also, previously vaccinated in infancy, the first result was again a vaccine pustule, but considerably modified. After this what becomes of the destructive agency of the chancrous virus? What becomes of the impossibility of imparting the two diseases at the same puncture?

A further argument adopted by Mr. Simon,* as well as by Hebra and Paget, is thus stated:—"To say 'this is a typical Jennerian vesicle' is, I believe, tantamount to saying, 'This is a vesicle which only one unmodified influence can produce, which no second influence can concur in producing, and in the contagion of which no second principle of infection can possibly reside'" (Simon).—"Every morbid appearance on the cutaneous envelope has its own peculiar characters by which it may be distinguished from other similar phenomena. The vaccine vesicle presents, in like manner, sufficiently striking peculiarities as to form, size, number, locality, and particularly as regards its course, to enable the observer easily to establish a distinction between the same and other vesicular bullæ or pustular eruptions" (Hebra).—"In short, the whole pathology of specific diseases will justify the assertion that a well-formed vaccine vesicle is certain proof of a pure and unmixed vaccine lymph, and that any modification of the lymph, by mixture of another virus, would be indicated by a corresponding modification of the vesicle" (Paget). Admitting, for the sake of argument, that this is true, will these authors maintain that it is absolutely true in every stage of the vaccine vesicle; as, for instance, when, the disease having arrived at its climax, changes are begun which are

* 'Papers,' &c., lxiii, pp. 136 and 138.

common to all similar eruptions at their decline, when the lymph is turbid, or when the seat of the vesicle has become that of common inflammation, the pure lymph of the specific malady having¹ become mixed with products not specific of vaccine? But Sperino's observation shows that the vaccine vesicle may be perfect, and yet that a virus of chancre may be present at the spot. Would any one have dared to vaccinate from the characteristic vesicle on Sperino's subject?

The objection to the doctrine of transmission of syphilis by vaccine lymph on the ground of impossibility being thus disposed of, we may proceed to examine the cases of alleged transmission as collected by Viennois, and as recorded by other observers.

I may as well say at once, that where a number of subjects have been vaccinated from a syphilitic subject, it has repeatedly happened that only a proportion have apparently received with the vaccine the virus of syphilis. A certain proportion—sometimes the larger proportion—have escaped the contamination. Doubtless this is an argument which the opponents of the doctrine of vaccino-syphilitic infection are at liberty to make the most of. Its supporters do not deny it. *They only claim that the possibility shall be allowed*, not the *necessity* of the transmission of syphilis. Heim, it is said, revaccinated some young women with virus taken from officers who had syphilis, and without any evil result; and also, with similar immunity, vaccinated several children from a syphilitic child. The supporters of the doctrine are quite willing to accept this or any like result as true, and even say as much of some of their own inoculations. But still they say that they have cases to prove that, on the other hand, syphilis *may* be transmitted along with the vaccine. Now, do the cases they narrate bear out their assertion? Will they bear the reasonable scrutiny which we may fairly ask

that they shall bear? With regard to a large proportion of them we must reply in the negative. There is no one of the numerous cases recorded by Viennois in which *all* the requirements which may be considered essential to *absolute proof* of transmission by the vaccine lymph, and by that alone, is met.

Some of my readers may be disposed to stop here. But, although all the proof that we require may not be forthcoming, we must recollect that there may still remain a *probability* that the transmission took place as alleged, and probability has its degrees. For example, the purity of the hands and of the lancet of the operator is not mentioned in any of Viennois's instances, except one where a new lancet is stated to have been used; but it is likely, nevertheless, that if they had been foul all the subjects vaccinated by the same operator and at the same time would have been equally contaminated; and in such instances of vaccination of several individuals from the same source some always escaped the syphilis. And, again, if the child that was vaccinated was healthy, as it must have been to be capable of successful syphilitic inoculation from any source, the probability is that the parents and nurse were healthy too, or why should the contagion have been deferred until after the punctures were made? Why should the chancre have appeared at the seat of the punctures, and there only? Surely other parts were liable also to receive syphilitic contagion. Thus far we may, perhaps, allow a little relaxation of the stringency of our requirements, but our other demands are imperative. *Are they complied with?*

An approach to compliance is found in two cases of re-vaccinated soldiers related by M. Guyenot, and of which further particulars are supplied by M. Lecoq;* but the vaccine did not run its unmodified course, and the incu-

* Viennois, 'Archives Générales,' 1860, p. 33.

bation was so short that we must suspect a contamination from a primary chancre. Besides, with all the assurances that we may receive, there will always remain a doubt about adult instances.

Dr. Whitehead's cases are all recorded too loosely to possess any value, but as he has based upon them a statement respecting the danger of syphilitic contamination, which I believe is totally unwarranted by the facts he adduces, and as he is the only English author of any standing who maintains the frequency of vaccino-syphilitic inoculation, it becomes absolutely necessary that I should submit his assertions to a rigid examination. I am not aware that he has retracted his opinion by any public counterstatement, and his cases, imperfect as they are, are quoted by Viennois in proof of the position he assumes. The following is the statement of Dr. Whitehead to which I allude:—"The occasional presence of eruptions on the skin and other forms of disease, as an entailment apparent or actual of vaccination in a family not previously subject to such affections, undoubtedly operates in the minds of many very much to the depreciation of the procedure as a preventive and healthful measure; and certainly, in not a few instances, there would seem to be just and sufficient reason for such prejudice. But the cause of this is not to be found in the vaccine virus in its pure state; it is due to a morbid material superadded, in its nature peculiar and extraneous. *The noxious matter commonly conveyed by vaccination is the syphilitic poison.* A child of naturally vigorous constitution, whose blood is tainted with the poison of syphilis, may retain the outward appearance of health up to three, six, or twelve months, or even two or three years longer before a characteristic outbreak shows itself. The parents of such a child may also have the semblance, to superficial observation, of faultless health, although still possessing the seeds of this malady in a degree

sufficient for its transmission to their offspring. *It is from such sources that mischief is often derived and disseminated by vaccination*, and other modes of implantation ; and it is thus that the efficacy of this great sanitary measure has been *in many instances* rendered questionable.”* Now, if this statement were true, if Dr. Whitehead’s cases as recorded in a further page of the report bore out his assertion, it would be one very properly received with great alarm. But I have no hesitation whatever in saying that it is not true, that Dr. Whitehead’s cases do not bear out his assertion ; that he was in all probability led astray by loose statements such as hospital patients are very apt to indulge in, and that he misinterpreted what he observed himself. Let us return to the record given of his cases.† He furnishes us with a table of sixty-three cases of infantile syphilis, treated in the course of twenty-two months at the hospital, and out of these cases he attributes no less than fourteen or twenty-two per cent. positively to vaccination. Now, this is a proportion so enormous that it is *primâ facie* incredible : for either other observers, who, like Acton, or Erasmus Wilson, or De Meric, or Henry Lee, are constantly treating cases of infantile syphilis, must be exceedingly blind to have overlooked so common a cause of the disease, or syphilis must be rampant in Manchester as it certainly is not in London. We approach, then, the narration of his cases with no advantage of antecedent probability. I will detail them as given in the table, merely throwing them from a tabular into a narrative form. I will not take them precisely in order as they come in the table for a reason which will presently appear. The numbers are those appended to them on the table.

* ‘Third Report of the Clinical Hospital of Manchester,’ p. 52. I have italicised the passages in which the frequency of the accident referred to is expressed. There can be no doubt of what Dr. Whitehead means.

† Op. cit., p. 92.

"CASE 8.—An infant, aged eleven months, of bad habit of body. Roscola appeared after vaccination at the age of one month. When seen, there were flat mucous tubercles around the anus, great emaciation, and a syphilitic pallor. Slight relapses occurred after the symptoms under treatment had been absent for nine months; mucous tubercles and diarrhœa and retarded development. The father declared that he had never had any venereal affection, and the mother died of puerperal fever one week after delivery."

"CASE 10.—An infant, aged twenty-three months, of good habit of body, had eruption appear on the face, neck, and ears immediately after vaccination, at the age of four months, followed by parotid abscesses on both sides; after the abscesses, eruptions and intertrigo until the appearance of chancrous ulcers. When seen, there were large excavated chancrous-looking ulcers on each labium pudendi near the upper commissure, with hardened base, raised edges, and lardaceous coating, and surrounded with dark erythema and a number of eczematous spots; purulent discharge from the vulva; intertrigo inguinalis et aurium. The ulcers were at first believed to be primary chancres, but no sufficient cause could be detected. The father was said to be healthy, and the mother was apparently healthy."

"CASE 31.—An infant, aged fifteen months, of bad habit of body, when seen had serpiginous psoriasis on the thighs, breech, and hands, was atrophied, and had chronic bronchitis, with a suspicion of tubercles. These symptoms appeared soon after vaccination at the age of three months. The health of the father was unknown. The mother was apparently healthy."

"CASE 35.—An infant, aged three months, of bad habit of

body. Roseola appeared after vaccination at the age of four weeks. When seen, there were copper-coloured blotches all over the body, most thickly on the nates, ulcerated near the anus; it consists of maculæ, papulæ, and psoriasis; there were also aphonia, otorrhœa on the left side, and atrophy. The health of the father was not examined. The mother appeared healthy."

"CASE 36.—An infant, aged eleven months, of good habit of body. When seen, there was eczema of the entire scalp, face, and arms, and elsewhere; skin where free had the syphilitic sallowness, minutely wrinkled, furfuraceous; there were adenitis cervicalis, husky voice, and chronic laryngitis. These symptoms first appeared after vaccination at the age of four months; a second vaccination having been practised under the belief that it would remove what the first seemed to have caused. Father and mother healthy."

"CASE 38.—An infant, aged fifteen months, of bad habit of body. The child was healthy previous to vaccination, at the age of six months, but soon after blotches came out. When seen, there were eczema of the scalp and face, scattered copper-coloured, hard tubercles over the whole body and limbs, adenitis inguinalis, atrophy, vomiting, and diarrhœa, ulcerated tubercle of the right labial commissure, husky voice, cracked psoriasis around the anus, and syphilitic pallor of the skin. The father is described as a man of probity, in robust health, sober, and of thrifty habits; he declared that he had never had a syphilitic affection. The mother is said to have been healthy."

"CASE 40.—An infant, aged ten months, of medium habit of body. When seen, there were observed syphilitic pallor and œdema of the face, dry cracked ulceration of the eyelids,

nostrils, and lips ; a dark scaly ulcerated patch in the left axilla, intertrigo of the groins, soreness of the vulva and bronchitis. These symptoms came on after vaccination at the age of six months. Father and mother healthy."

"CASE 49.—A child, aged eleven years, of good habit of body. When seen there were observed sore throat, husky voice, nocturnal pains of the limbs and head and rigors. Is stated to have been healthy until vaccination was performed at the age of four months, but this was followed by deep ulcers and erysipelas, which persisted for six weeks ; twelve months later there were syphilitic eruptions on the scalp with glandular abscesses in the neck and blepharitis, which lasted two years. Father and mother healthy."

"CASE 58.—An infant, aged fifteen months, of medium habit of body. When seen there were lichen syphilitica, syphilitic pallor, copper-coloured erythematous blotches on the back and chest, a flat tubercle on the left cheek and atrophy. The symptoms set in after vaccination at the age of three-and-a-half months, up to which period the child was healthy ; they have continued to increase ever since. Father and mother apparently healthy."

Now, here are nine out of the fourteen cases of syphilis* asserted by Dr. Whitehead to be due to the vaccination. I maintain on the other hand that they were not, so far as the record permits me to judge, cases due to the vaccination at all, in the sense that the "noxious material," "the syphilitic poison," was "conveyed" to the children by the vaccination they underwent. I say that to assert this is to ignore altogether the teachings of observation and experiment, and,

* I assume the diagnosis of the syphilitic nature of all these cases to be correct.

moreover, is incalculably mischievous. Let us apply our test to them. When syphilitic virus is inoculated with the vaccine, a chancre appears at the seat of puncture *first* of all, afterwards the constitutional phenomena. When a latent inherent syphilitic taint is merely roused into activity by the constitutional disturbance natural to the development of the vaccine disease, the symptoms which appear are general constitutional phenomena. To which of these categories, then, do the nine cases related belong? Clearly not to the first, for there is not a word in any of them which will indicate that a chancre or anything like a chancre appeared at any time at the former seat of the vaccine vesicle. No. If the syphilis was consequent upon vaccination at all, it must be held that the children were previous to vaccination constitutionally infected, and that the only part the vaccination had in producing its manifestation was that it tended to rouse the latent taint into activity a little sooner perhaps than would otherwise have been the case. But we have no proof offered us that the symptoms mentioned were even consequent upon the vaccination. The cases are very briefly and loosely recorded, and Dr. Whitehead does not tell us his authority for the statements that the phenomena observed followed upon the vaccination. Were these children that Dr. Whitehead vaccinated himself, and so had the opportunity of observing the events which immediately followed, and of satisfying himself that none of the symptoms of syphilis existed prior to the vaccination? or did he derive his information on this head from the parents of the patients? This is a matter of some importance in view of authenticity, for popular notions lean to the side which refers every infantile disease to vaccination if it be observed about the same period of time. As to the statements about the health of the parents, unless it be distinctly specified in what way the healthiness and freedom from syphilitic taint was established,

every medical man who has any experience in the matter will take them *cum grano salis*.

But what about the remaining five cases? Four of them are quoted by Viennois as appearing to him to be due to the vaccination.

“CASE 2.—An infant, aged nine months, of a bad habit of body. Copper-coloured blotches appeared after vaccination at the age of five months. When seen, there was a mixed eruption on the face and scalp and extreme irritability of the whole surface; the vaccinated spots remained unhealed at the end of five months, presenting a well-formed rupia with excavation. The father and mother are described as apparently healthy.”

“CASE 11.—An infant, aged eleven weeks, of medium habit of body. When seen there were two deep ulcers with hardened bases where the vaccine vesicles were formed three weeks previously; copper-coloured roseola on the nates and chin, sallow complexion, mucous tubercles around the anus, eruptions and intertrigo behind the ears, coryza, atrophy, and dysentery. The history of the case is that roseola appeared from twelve to fourteen days after the vaccination, at the age of two months, the mucous tubercles nine weeks after, while under treatment, and atrophy four months later. Father said to be healthy; mother feeble, but apparently free from taint.”

“CASE 14.—An infant, aged seven months, of bad habit of body. It is stated that this child was healthy until it was vaccinated at the age of three months; that the vesicles were angry and festered, and were followed by blotches and wasting. When seen, there were ulcerative erythema of the breech and nates and psoriasis of the anus; thighs covered

with copper-coloured blotches of serpiginous arrangement, syphilitic pallor, senile expression, great atrophy, stomatitis erythematosa, and husky voice. The father, a man of character and probity, declares that he never had a venereal affection. Mother apparently free from taint."

"CASE 56.—An infant, aged seven and a half months, of good habit of body. After the subsidence of the vaccination, at the age of two months, the vesicles degenerated into ulcers, surrounded by erythema. When seen, there were erythematous blotches of a copper colour on the chest and neck, eczema auris, arthritis of left elbow-joint, herpes tonsurans, and syphilitic pallor. Father said to be healthy; mother apparently healthy."

"CASE 57.—A child, aged three years and three months, of good habit of body. It is stated that she was healthy up to the time of vaccination, three months ago. The three vaccinated spots degenerated into three deep ulcerations with hardened bases, which remained open two months. When seen, there were all over the trunk and limbs flat herpetic-like crusts, with large erythematous areolæ of copper tint, most numerous on the thighs, the cicatrices of the first formed patches being of a deep copper colour. Has now great prostration, inappetence, enuresis, and dysuria, erythema of the vulva without discharge, chronic blepharitis, and photophobia, syphilitic pallor. The first symptoms were ulceration of the vaccinated spots with copper-coloured blotches. Father and mother apparently healthy."

These five cases are the only ones out of the whole fourteen in which even a suspicion could arise of syphilis having been imparted to the children at the time the vaccination was performed and in the act of vaccination. A suspicion—that is all. Only in two of the cases (Cases 2 and 11) does it appear

that Dr. Whitehead himself saw the ulcers at the seat of the vesicles; in the other three it does not appear that he saw them. And of those he did see, in only one is the induration of the base of the sores mentioned, and in neither is there any mention made of any corresponding glandular enlargement. In the other three cases the sores might have been primary chancres, or they might not. Supposing, for the sake of argument, that they were, a fact which I cannot admit in the absence of any evidence, there is still no proof offered that the syphilitic virus was introduced by the vaccinating lancet. There might, for anything that seems to have been actually ascertained, have been some other medium of infection. No trouble seems to have been taken to discover whether there was any communicable lesion about the fathers or mothers or other persons who came into contact with the children, nor yet to discover under what circumstances and by whom the vaccination was performed. And as to the source of the lymph accused of having conveyed the syphilis, nothing appears on the record at all—no inquiry whatever seems to have been instituted; but because sores appeared at the vaccinated spots, whose primary chancrous nature is even questionable, and then syphilitic lesions of a constitutional character, the conclusion is leaped to that the vaccination introduced the syphilitic virus into the inoculated spot. The most improbable of all the conceivable modes of communication is inferred, without a tittle of evidence pointing to it, and without first ascertaining that the more ordinary modes of communication were not likely to have been operative.

And on these five cases, thus imperfectly observed and unsatisfactorily recorded, and upon nine others about which no doubt can exist in the mind of any person who is familiar with the results of modern syphilitic research, that they were cases of constitutional taint previously existent, but in which the vaccine disease, as any other constitutional disturbance

might have done, roused the inherent syphilis into activity, Dr. Whitehead bases the inference that "the syphilitic poison is commonly conveyed by vaccination." I say that his fourteen cases will bear no such inference—that his conclusion is totally unwarrantable.

Another case is recorded by Dr. Whitehead, in his work on hereditary diseases,* the heading of which is "Secondary Syphilis communicated by Vaccination—fatal consequences." In this case Dr. Whitehead performed the vaccination himself from a neighbour's child, selected by the parties themselves, but on whom there were at the time erythematous blotches, the syphilitic nature of which he did not recognise at the time. On the ninth day "the vesicles were unusually large; each sore (*sic*) was surrounded by deep and extensive inflammatory hardness, the redness extending around that part of the limb, and the axillary glands were enlarged and tender on both sides. About the twelfth or fourteenth day both wounds were deep, broad ulcers, with extensive induration; the glandular swelling in the right armpit had acquired a fearful size, and seemed disposed to suppurate. On the twentieth day this swelling had become an extensive abscess, which, on being opened, gave exit to a large quantity of offensive matter; the vaccinated spots were angry-looking excavated ulcers, of chancrous aspect, with no apparent tendency to heal. A number of copper-coloured blotches were at this period present on the skin; the mouth was inflamed and excoriated, and appeared to have communicated a similar lesion to the mother's nipple, with other disturbances to be noticed presently. The child had been hitherto treated by simple remedies; *but having by this time obtained information which led to the belief* that the infant from whom the vaccine virus had been obtained laboured under the syphilitic taint,

* 'On the Transmission from Parent to Offspring of some forms of Disease, and of Morbid Taints and Tendencies,' 1851, p. 174.

I prescribed mercurial inunction and small doses of Hydrargyrum cum Cretâ, which seemed for a time to be attended with benefit. Shortly, however, the patient's elbow-joint became swollen and inflamed, and several periosteal swellings came out on the front of the right leg, one of which formed into an abscess, which never healed, the subjacent bone being extensively denuded. The infant died at the age of about four months and a half.

"It appeared upon inquiry that the infant from whom the vaccine matter was taken had suffered soon after birth from purulent ophthalmia, accompanied by blotches on the skin, for which it was several weeks under treatment. Its mother was said to have had gonorrhœa, contracted from her husband during pregnancy, but of which she believed herself quite cured. I had not an opportunity of learning the subsequent history of these parties.

"The case of the mother of the deceased proved to be one of a very distressing kind. The breasts with the nipples and superficial absorbent vessels became so inflamed as to necessitate the discontinuance of their use in nursing. The disturbance was most severe on the left side, extending to the axilla, in which situation an extensive abscess formed, which gave exit to an incredible quantity of offensive purulent matter. As the abscess contracted, blotches of roseola came out in various parts of the body, and these, I remember, continued for a considerable period, varying with the state of functional health prevalent at the time; but she was ever afterwards an invalid. She constantly complained of rheumatic pains of the head and limbs, especially troublesome in the night time, and of urethral irritation implicating the neighbouring parts. The cellular structure, embracing the urethral canal, was enormously thickened, hard, and exquisitely tender; the whole vagina was inflamed and highly irritable, and there was a plentiful sanious leucorrhœa, which afterwards became

very offensive. At a later period a deep-seated swelling began to appear in the left hypogastric region; the lower division of the uterus was at the same time enlarged, nodulated with irregular, firm elevations, encroaching upon the vaginal cavity, painful under slight pressure. As the abdominal tumour increased in size she became dropsical, and died April 30th, 1842, at the age of thirty-eight years, about three years after the invasion of the mischief occasioned by the vaccination. I made an examination of the body the day after death. The left ovary was the size of a child's head, of unequal density and structure, being on one side of schirrhoid hardness, and elsewhere multilocular; the cells, as well as the Fallopian tube, being filled with soft cancerous matter. The whole uterus was schirrhoid. The husband died of tubercular phthisis a short time before the decease of his wife."

This case is narrated more fully than those which I have quoted before, but yet not fully enough to carry conviction to the mind. Still, the narration is such as to lead a non-professional person to infer—what I cannot believe the author intended to be inferred—that the mother's uterine and ovarian disease, of which she died, was the result of syphilitic infection communicated to her from her child. This, at all events, had nothing to do with syphilis. But what as to the vaccinated infant? Was the disease which followed the vaccination really infecting syphilis? Dr. Whitehead evidently did not recognise it as such until something met his ear about the health of the vaccinifer, nor, indeed, is the narration at all convincing as he gives it to us. With the exception of the copper-coloured blotches, in themselves not pathognomonic,*

* "The copper colour of syphilitic eruptions, however, is by no means constant, and may be simulated by various forms of skin disease which are not dependent upon the syphilitic virus. . . . It is absent in most cases of syphilitic erythema at the commencement of the eruption, and only appears as the blotches begin to fade away; and as a general rule, in nearly all

the phenomena observed, the ulcerations, suppurating glands, inflamed joints, abscess of the leg, and denudation of the bone, carry to my mind as much the impression of scrofulous as of syphilitic disease. And the evidence of syphilitic infection in the vaccinifer is even weaker than in the case of the child vaccinated. Blotches on the skin (undescribed) and purulent ophthalmia by no means necessarily indicate syphilis, even though the mother might have had gonorrhœa. And then, to carry the commentary a step further, the inflammation and abscesses of the mother's breasts and a roseola (again undescribed) surely cannot be adduced as evidence of her syphilitic contamination from the mouth of the infant. Had this occurred it is a chancre which would have been imparted, and not a common inflammation terminating in abscess. The last few words of the narration seem to me to explain the whole affair. The father was tuberculous, and died of phthisis; the mother was the subject of constitutional debilitating disease.

Dr. Whitehead's cases are, then, all too loose to possess any value. So are those of Marcolini. Much stress is laid on two cases related by Cerioli, of Lucca. In the one instance forty-six children were vaccinated from an *apparently healthy* child, but this is all we hear about it; nothing about any previous examination of the child, and nothing as to its parents. Of the forty-six vaccinated from it, only six escaped syphilitic contagion. Where so large a proportion became infected, it is more than probable that the syphilitic virus was conveyed to them *at the time of their vaccination*. But from what source? Here we hesitate. Facts are wanting to exclude other sources than the vaccine vesicle. Nor do we hear anything about the subsequent health of the *foundling* infant who furnished the vaccine lymph. Similar and

syphilitic eruptions, the copper colour is less marked at an early than at a late period."—Bamstead on 'Venereal Diseases,' p. 551.

still more serious objections may fairly be made to the second of Cerioli's cases, where several children apparently received syphilis at the period of vaccination from a child *apparently healthy*, but whose father was syphilitic, the child herself presenting what were believed to be syphilitic symptoms at a subsequent period. Lastly, there is the celebrated Hübner case, which, if it teach us no other lesson, teaches this—that nothing but the assurance of a most complete personal examination of the vaccinifer should, in the record of a case, be accepted as positive evidence that there has not been some contagious manifestation of syphilis upon him. In this instance it was stated by Hübner that the vaccinifer was healthy, just as in Cerioli's cases; but the mother, Marguerite Keller, contradicted him in court (for the case was the subject of proceedings at law), when called upon for her evidence, and declared that there were, even then, pustules upon the legs of the infant. It appears that, at the time of her confinement, the mother herself had suspicious ulcerations of the mouth and genitals, but these had been cured; and an infant who slept in the same bed with Keller and her child did not become the subject of syphilis. Eight children were vaccinated from this child; the punctures ulcerated after a period of incubation; and all these children, from the evidence of those who visited them subsequently, were declared to have been contaminated with syphilis. Five others, vaccinated at the same time, from the same child, escaped. It is worthy of observation that a child vaccinated from one of these five is said to have become syphilitic, yet the subject which furnished the vaccine had remained healthy for ten months after its vaccination. Does not this strengthen our conviction of the importance of recording the state of health of the persons about the vaccinated child said to have been infected by vaccination?—for it cannot be said that the subject from whom it was vaccinated had been syphilised, or

had been labouring under congenital syphilis, which would probably have declared itself within ten months.

I need not go further to show what I hold to be the imperfections and fallacies of these records, collected by Viennois and reproduced in the work of M. Rollet. There is not one which does not present some omission, or several omissions, fatal to its reception, if the object of those who have adduced them be to exhibit *conclusive proofs* of their position. I admit that the probabilities are in their favour, and that their view is strengthened by the following remarkable facts, which strike me in the history of this controversy:—1. That, notwithstanding the rejection of the view maintained by those who have observed cases of alleged transmission, they who reject the explanation offered have in no instance that I am aware of established the source of the accidental contagion by which they would explain the occurrence of the syphilis. Those who hold the doctrine of M. Ricord certainly are not few, and no one can accuse either them or their master of a lack of enthusiasm. Let me relate a case which happened in Paris. A young woman, aged eighteen years, under the care of the veteran M. Trousseau, in the Hôtel Dieu, was vaccinated, at the same time as four children, from a child apparently in perfect health. She was vaccinated the *last* of the batch, and the lymph was taken from a vesicle as young as the fifth or sixth day. At this time the quantity of lymph obtainable is small, and M. Trousseau thinks ('Bull. de l'Acad. Sciences,' Jan. 25, 1865) that it is pretty certain that the vesicle had been sufficiently injured by the time this young woman's turn arrived to have furnished some blood in admixture with the lymph. Nothing abnormal had appeared either on the children at the time of their leaving the hospital or on the vaccinifer, but on the young woman the re-vaccination did not succeed, and when she left the hospital on the eighth day all trace of the punctures had disappeared.

However, at two out of six of the punctures made there appeared, by the twenty-third day, unmistakable proof of the implantation of syphilis. M. Ricord came forward and delivered two clinical lectures upon this case. How does he explain the phenomenon? He does not deny the *possibility* of the syphilis being conveyed by the vaccination, as probably he would have done formerly; but, while throwing theoretical doubts upon it, he fails, even in this case, occurring under his own eyes, and in which he had every opportunity of discovering the source of contagion, if other than the vaccination, in fixing satisfactorily upon any other explanation. He merely suggests that as she was married and had left the hospital she might have got syphilis from her husband *by her arm*. Why there, and not elsewhere? M. Trousseau, however, had frequent opportunities of seeing the woman's husband, and ascertained, as a positive fact, that he was not suffering from syphilis ('Bull. de l'Acad.,' Jan. 25, 1865). Another suggestion made was that the woman inoculated her own arm by scratching—suffering at the time from a concealed intra-uterine chancre. But she was shown to have no such affection, and besides, if she had, how could a non-auto-inoculable disease be thus transferred? M. Ricord relates, indeed, a number of cases to show that syphilis may attach itself to a person from no one knows where. But here was a case in which, at any rate, it might have been established whether the contagion had been received through the medium of disease in the husband, or how. The record of the case also fails to indicate whether or not syphilis appeared upon any of the children vaccinated at the same time, when the period fairly allowable for incubation had elapsed. At a later period, when the subject of vaccinal syphilis was being discussed at the Academy, M. Ricord seems to have felt more deeply the difficulty of accounting for this case other than by vaccinal infection. He says—

“Ce fait, en raison de ces circonstances, restera, pour moi, un cas probable, *très probable*, je le veux, *de contagion vaccinale*, mais rien de plus : la certitude n’y est pas.”* I must say that, admitting all that M. Ricord adduces, if the promoters of the affirmative doctrine have failed to do their best in placing it on an incontrovertible basis, their opponents have equally failed in the performance of the task which devolved upon them.

2. It is to my mind strange that the only cases of communication of venereal affection recorded should be instances of alleged transmission of the true infecting syphilis. Is “chancroid” so rare that the opponents of Viennois’ views can adduce no cases to show that the punctures are now and then the seat of chancroid? If they desire to prove that local accidental contagion was the real cause of the apparent transmission by the vaccine, surely this would be an argument of value. The neglect to adduce it is to my mind greatly in favour of the hypothesis of Viennois, that the syphilis really gets into the system *somehow* from the *interior* of the vaccine vesicle, and not from some local lesion, the virus of which has come casually into relation with the punctures.

Three cases are recorded by Mr. Geo. Nayler in his work† to show that syphilis may follow vaccination, but they neither show nor is it attempted to be shown that the syphilis was communicated from a syphilitic vaccinifer.

But the instances of reputed vaccino-syphilitic inoculation which have produced the greatest impression upon the public mind are those in which vaccination has been followed by syphilis in a number of children vaccinated from the same source, and where the disease has spread to mothers, nurses,

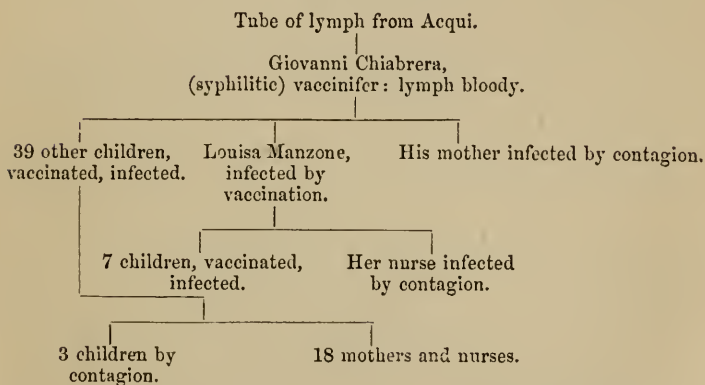
* ‘De la Syphilis Vaccinale: Communication à l’Académie Impériale de Médecine,’ 1865, p. 32.

† ‘A Practical and Theoretical Treatise on the Diseases of the Skin,’ 1866, p. 279.

brothers, sisters, &c., in the locality, after the manner of an epidemic disease. Such instances have been truly termed *disasters*. *Four such disasters* have been more or less carefully recorded, and I propose now to consider them, and to inquire how far they afford evidence of the fact of vaccino-syphilitic inoculation, or whether the occurrences are capable of any more probable explanation.

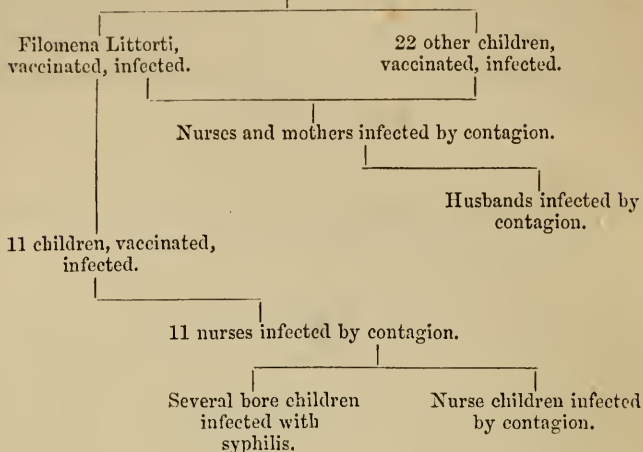
* GENEALOGICAL SCHEMES OF FOUR SERIES OF REPUTED
VACCINO-SYPHILITIC INOCULATIONS.

1. *Rivalta series.*



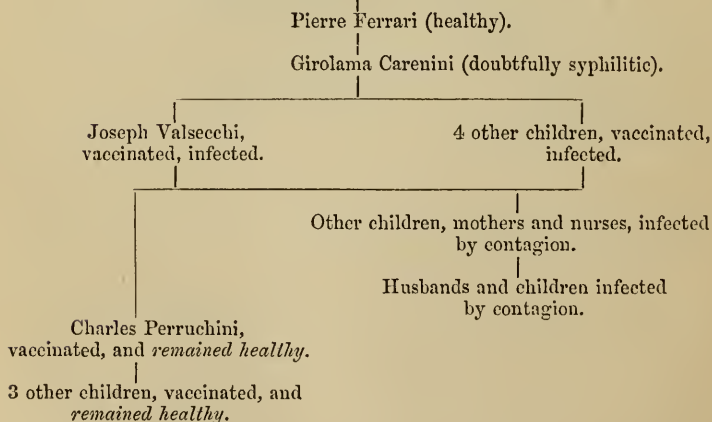
2. *Lupara series.*

Tube of lymph from Campobasso, bloody, vaccinifer syphilitic (?).



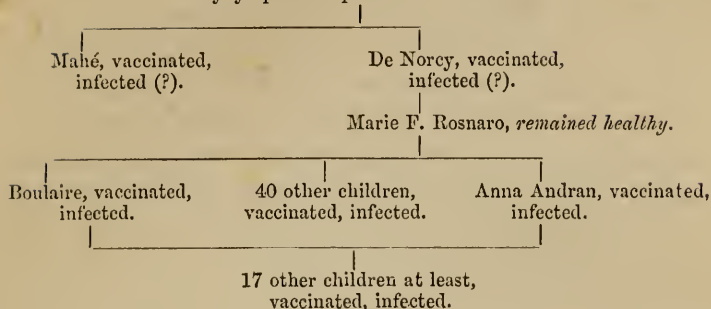
3. *Bergame series.*

Preserved lymph from previous season.



4. *Auray series.*

Dry lymph from prefecture at Vannes.



1. *The Rivalta series.*—This series was not, indeed, the first in point of date, but it was the first to attract public attention by the magnitude of the disaster. The particulars were first furnished by M. Cerisse,* whose information has since been supplemented by a commission of inquiry instituted by the Government. I will abridge the account as it may be gathered from the various sources at our disposal as much as is consistent with perspicuity.

On May 24th, 1861, a child, residing with his parents at Rivalta, named Giovanni Chiabrera, was vaccinated by a surgeon named Cagiola with lymph preserved in a tube sent to him from Acqui. He was eleven months old. His previous history is that two or three months before his vaccination he had been used to draw the breasts of a woman unquestionably affected with syphilis. His father, although said to have been the Don Juan of his village, containing a population of about 2000 souls, presented no trace of any recent or former syphilitic infection. Indeed, according to the account given, syphilis was a disease unknown up to this time in the place. That the child's mother was not syphilitic is shown by the fact that at a later period she was infected from her

* 'L'Union Médicale,' November, 1861, p. 259, to February 20th, 1862.

own or some other child. Chiabrera, then, was most probably infected with syphilis to start with. The vaccination, as might have been anticipated in such a case, pursued a regular course. On the tenth day from vaccination, however, he began to suffer from diarrhœa, which, continuing, led to a marasmus, of which he died in the following October. It was in this month of October that the commission made its inquiry, and it was found that the mother of Chiabrera had then primary syphilitic ulcerations beneath the nipples. The child was also found to have some erythema about the anus, which the diarrhœa might account for, as well as on the prepuce, and a little excoriated papule on the free border of the latter; the inguinal glands were swollen and hard, and he suffered from alopecia. It is remarkable that the father was found at that time not to have been infected. If we say that the child imparted the contagion to the mother, the great difficulty about the case is that nothing was found on examination of the child's mouth. But still, even if she had got syphilis by suckling some other child, the freedom of Chiabrera's mouth would show that he was already constitutionally affected. Put the case how we will, we cannot escape the inference that Chiabrera, either by hereditary transmission (which is not very likely) or from the woman he sucked (which is probable), or in some other way, had become *constitutionally syphilitic*, and was so at the time when his vaccination was performed.

On the tenth day, that is, on the 2nd of June, forty-six other children were vaccinated from Chiabrera (the *first* vaccinifer of the series). Of these, thirty-nine became affected with syphilis, the seven who escaped being among the *last* of the batch vaccinated from him. According to Dr. De Katt, the syphilitic symptoms showed themselves at periods varying from ten days to two months, the mean time being twenty days after vaccination. They consisted first of all in

ulcerations at the seat of the vesicles, with specific glandular enlargements, and then, subsequently, mucous tubercles, ulcerations of the mouth and throat, cutaneous eruptions, &c. Some of the children died.

Among the thirty-nine infected from Chiabrera was Louisa Manzone (the *second* vaccinifer of the series). She was aged six months. *On the tenth day*, again, she was used for the vaccination of seventeen other children, of whom seven became affected with syphilis, suffering in the same manner as the thirty-nine infected from Chiabrera. Two months afterwards she was taken to Acqui, and seen by Dr. Silventi, who recognised upon her a syphilitic eruption, with mucous tubercles about the anus and vulva, mucous patches on the commissures of the lips, and indolent glandular enlargements, the primary affection at the seat of the vesicles not even being healed. The child died in September. The nurse who suckled her got ulcers on the breast, and from the various other children who were syphilised the disease spread by contagion to eighteen mothers or nurses and to three other children.

Now, how are we to account for all this? Syphilis was not prevalent in the village at the time, and Chiabrera does not appear, at the time he was vaccinated, to have been actually suffering under any of the constitutional manifestations of the infection he had undergone; so that I do not see how the infection of the thirty-nine children vaccinated from him could be attributed to anything but the introduction, with the vaccine virus taken from his arm, of the syphilitic virus at the same time, and that both were taken from the same source. With regard to the second vaccinifer, Manzone, what we have specially to remark is that if the seven children vaccinated from her, and subsequently suffering from syphilis, were also, as is most probable, actually infected in a similar manner to those infected from Chiabrera, we must admit and

explain as best we may the fact that the lymph taken from Manzone's arm communicated syphilis at the time when Manzone herself, not having yet undergone the primary manifestation, *could not have been constitutionally infected*. This appears to have been one of M. Ricord's difficulties in regard to the Rivalta series. The explanation which I suggest is this, that the syphilitic virus from Manzone, which operated in the seven vaccinated from her, was not a virus manufactured in Manzone's system. Clearly it was not, could not be that, but was *a portion of the identical virus obtained from Chiabrera* placed originally in the punctured spot, and undergoing there those local changes and possibly that *local reproduction* which would ultimately result in the chancre when the vaccine disease had come to an end. Possibly this explanation, if accepted, may account further for the fact that only seven out of seventeen vaccinated from her were syphilised; whereas thirty-nine out of forty-six vaccinated from the constitutionally affected child Chiabrera were syphilised. There must have been some cause for this great disproportion; it may lie in the fact (partly) that in Manzone's case the vesicle was not in all parts equally impregnated by the virus. It is to be recollected that the operation of the virus is not dependent upon quantity, but upon quality.

2. *The Lupara series*.—This series of reputed vaccino-syphilitic inoculations occurred in 1856, but was kept secret until the Rivalta series had been made public, and the subject was undergoing discussion in medical circles. M. Marone, in whose practice it occurred, and who published his account of it in 1862,* states that he had not dared to make it known before, partly because of his own reputation, and partly because he feared that he should injure that of vaccination in general. The publication of the Rivalta cases showed him that he should keep silence no longer. The following is the

* 'Impraziale de Florence,' No. 5, 1862.

summary furnished by M. Depaul:—“At the close of October, 1856, M. Marone obtained some lymph in tubes from Campobasso; it was transparent, but tinged with blood. A large number of infants were vaccinated, and twenty-three of them were infected with syphilis. He gives the names and the ages, which last varied from five to ten months. All these children, as well as their parents, were healthy at the time of the vaccination. The vaccinal eruption proceeded regularly up to the period of desiccation; but then the dry crust began to soften anew, and when it fell off an ulcer with indurated base was found to occupy its seat. In other cases the dry crust remained adherent for an unusual length of time, and then fell off. At the expiration of a few days the cicatrix reopened, and a sore was produced, having all the characters of an indurated chancre, and lasted a month or six weeks. In all the subjects indolent, non-suppurating, enlarged glands in the axilla were observed. In all of them, also, towards the middle of January, the general symptoms of syphilis appeared—roseola, papules, pemphigus, mucous patches on the lips, mouth, anus, and genitals, with inguinal and cervical glands engorged. The nurses were infected in their turn with indurated chancres on the breast; and, at a later period, that is, after five or eight weeks, they had also general symptoms, roseola, psoriasis, impetigo, mucous patches, &c. In their turn, also, the mothers imparted the disease to the fathers. M. Marone has since learned that the vaccine sent from Campobasso had been furnished by a little girl who died some time after vaccination from an eruptive affection, the character of which, however, has not been thoroughly determined.

Filomena Littorti, one of the twenty-three infected, served for new vaccinations. Eleven infants were contaminated. First they had the characteristic primary affection, and then

* ‘De la Syphilis Vaccinale,’ &c., p. 108.

the consecutive phenomena. Mothers were again contaminated, and they infected others. The disease having been at last recognised, a specific treatment was instituted; but, nevertheless, several of the children died. Eleven nurses, infected by the vaccinated children, infected, in their turn, other children who were not vaccinated, but who had been suckled by them. Several of the contaminated women, too, having fallen pregnant, were confined either prematurely or at full term of children, dead or living, but *in every case bearing the marks of congenital syphilis.*"

This, again, is a series where the probabilities are quite against the idea of accidental or fortuitous infection, syphilis prior to 1856 not having been observed by M. Marone among the peasants of Lupara. It is pretty nearly the counterpart of the Rivalta series. It is defective, however, in some points. We learn nothing absolute about the child from whom the lymph was taken; but it is tolerably certain that the lymph itself contained, *somehow*, syphilitic virus (it might have been accidentally), and absolutely certain that first vaccinia and then syphilis resulted from its use. In favour of the hypothesis that the child was syphilitic, there is the fact that, putting syphilis aside, there are very few eruptive diseases of infancy (except the exanthemata) which are of a fatal character. It is to be regretted that the proportion of those vaccinated in the first and second generations with the Campobasso lymph who became infected is not stated, together with the day of eruption on which lymph was taken from Littorti. Still, we meet here with a similar occurrence to what we saw in Manzone's case, namely, the transmission of syphilis prior to its constitutional development, and as I believe simply *mediately*.

3.—*The Torre de Busi or Bergamo series.*—This series was reported to the Conseil de Santé of Bergamo, in 1863, by Dr. Adelasio, and is included in a paper read to the Medical

Congress of Lyons by M. Viennois.* The account is a very long one, and I shall therefore content myself with giving an abstract of it.

In the autumn of 1861 M. Quarenghi, a physician at Torre de Busi, vaccinated Pierre Ferrari with lymph which had been preserved from the last season six months previously. The pustules were well developed, and presented all their normal characters; so good were they that they were selected for the vaccinations of the following season. In none of the subjects vaccinated at this time did the vaccination produce any morbid complications. It is to be observed that Ferrari died a year afterwards, it is said of severe cough (*toux férine*). The parents, when examined in April, 1863, were found perfectly healthy, and they had a repute of being moral.

On May 8th, 1862, Girolama Carenini, born of parents young and in perfect health, was vaccinated with the lymph of Ferrari, preserved in tubes. It does not appear on the statement that other children were vaccinated with the same lymph. It is stated by *the mothers* of the children who were vaccinated from Girolama that, although she was plump and of a good colour, they noticed here and there upon her an eruption, which, by the description given, was something like chicken-pox. The pustules from which the lymph was taken were fine, and the operation itself was performed *with a needle*, without any blood being drawn. When the child was examined subsequently, five natural cicatrices were found, and there was nothing abnormal upon the body of the child. The parents were also carefully examined, and no trace of any syphilitic affection was found upon them. All that they would admit was that the child had got an eruption from exposure to the sun. Well, from this child, Girolama, six other children were vaccinated on the 15th May (eighth day), and out of the six five were infected with syphilis, but the vac-

* 'De la Syphilis Vaccinale,' &c., p. 303.

cinator could not recollect whether the one which escaped infection was vaccinated first or last.* In the case of the five infected it is stated that the fall of the crusts was delayed, and that indurated ulcers replaced them, which were followed by secondary symptoms, which were in no case typical, but left copper-coloured stains. About the same time mucous patches appeared, and the mothers and nurses were infected, and imparted the disease to their husbands. The disease spread also by contagion to brothers, sisters, aunts, and cousins. Altogether, there is a record of twenty-three persons whom these children infected.

And now comes the remarkable part of the recital (read in the light of other similar series). One of the infected children, Joseph Valsecchi, aged five months, who infected altogether five other persons, was selected on May 23rd for supplying lymph for the vaccination of five other children. It took upon all. In four of the five the healing of the pustules is said to have been rather tardy, and there was some alteration in the form of the cicatrices, and, in two, there had occurred a cutaneous eruption, which got well of itself, and did not affect the general health.

One of these five, Charles Perruchini, served for the vaccination of three more children, who remained perfectly healthy, only one of them presenting the cicatrix a little elevated in the middle.

I cannot regard this as a satisfactory recital. It bears on its face so much evidence of after recollection on the part of the vaccinator. No doubt that the investigations made into the health of the five infected from Girolama and their relatives is accurate enough. I think there is no doubt in the world that they had syphilis after vaccination with Girolama's lymph, but I am not at all satisfied about Girolama's syphilis.

* The editor adds a note to the effect that, if M. Quarenghi could not recollect this, how came he to recollect that no blood was drawn?

Nor am I quite satisfied that the children vaccinated on the 23rd of May were vaccinated from Valsecchi—at least, I believe that a man who trusted so much to memory was very likely to be mistaken as to the child he took the lymph from. It is much more probable that the vaccinator was careless and slovenly, and had infected the first series himself in some way. It is true the report goes that he used, not a lancet, which might have been used previously for some other purpose, but a needle; still, even here he might have made an incorrect statement in order to shield himself. All that this recital, in fact, can absolutely tell us is, that the two viruses, that of vaccinia and that of syphilis, may be received at the same spot, and may both, one after the other, be developed there. Besides, we are not told, as in the former series, that syphilis was unknown in the place previously.

M. Adelasio records an additional instance of infection that happened at Almé, in the same province as Bergame. "On September 14, 1863, in the Commune of L—, the child Charles Arrigoni served as vaccinifer to a whole series of children. The vaccine was regular in its course upon all of them. Among these children was the daughter of Dr. X—, a physician of the country. This child, in the previous month of April, had been sent to nurse to a woman, A. E—, apparently in good health, but presenting a fissure on the breast, said to have been caused by the biting of the teeth of her first child, but which had raised some suspicions in the mind of the midwife of the hospital. Three months afterwards Dr. X—'s child had upon the thighs and groins rounded rose spots, some of them flat, and others elevated.

On September 21st (eighth day) Dr. X— vaccinated from his daughter two children, Cornago and Corelli, some days after which the rose spots became more pronounced and more confluent, and a fissure of the anus supervened, with engorgement of the cervical glands, and he diagnosed syphilis, in

which he was confirmed by one of his colleagues. The child's nurse also had three ulcers on the genitals and indurations in the groins.

The mothers say that the vaccinal pustules of the doctor's daughter were fine, and that no blood flowed during the vaccination. The incubation of the vaccine was a little prolonged in the case of Cornago and Corelli. The crusts fell on the thirty-fifth day, and gave place to ulcerations. Towards the middle of November these two children had mucous patches on the buttocks, around the anus, and on the thighs, and rhagades."

This case, again, is altogether defective. No doubt Dr. X—'s child was syphilitic, but there is nothing to show that the infection of Cornago and Corelli was not fortuitous—that they received the syphilitic virus from the interior of the vesicle, and from no other source.

I cannot take these Bergame cases into account in the discussion.

4. *The Auray series*.—The account of this series is given by M. Depaul* in his official report to the Minister of Agriculture, &c., in France. Again I give it in abstract, leaving out nothing in any way, I believe, essential to the forming of a sound judgment. I may say that the first intimation of the occurrence was received by M. Depaul from Drs. Closmado and Denis, of Auray, who accompanied M. Depaul in his visit to the place three months afterwards, and that some additional facts were obtained by a member of the Academy, M. de Kergaradec. It appears that a midwife, named Madame Françoise Lemouel, an experienced vaccinator, who had on two occasions been rewarded for the zeal she displayed, was the vaccinator of the series. She received, on May 20th, 1866, some dry lymph from the prefecture at Vannes, with which, on the next day, she vaccinated two

* 'Bulletin de l'Académie Imp. de Méd.,' t. xxxii, No. 4, p. 201.

children "in excellent health," named Mahé and De Norey. Mahé, when visited by M. Depaul in August, was aged fifteen months, and had *two ordinary cicatrices* on the right arm and three on the left. A little indolent axillary adenitis existed on both sides. According to the account given by the family, he had been very ill during the five weeks succeeding the vaccination, and the pustules had supplicated during seven weeks. At the time of the visit, however, he was very well in health, although he had not undergone any specific treatment, and his mother had not suffered in any way.

Jean Marie de Norey, aged ten months, had, like the preceding child, been suckled by his mother. He was a large and fat child, and appeared in good health. He had on each arm two normal scars, only still a little red. He also had, on both sides, a little indolent axillary adenitis, cervical adenitis, but no eruption or other affection of the skin. The mother had not suffered in health, but, according to her and her husband, the child had been very ill for at least five weeks after the vaccination, the pustules supplicating during all that time. An eruption, which they called "measles," had appeared three weeks after vaccination.

De Norey was used as vaccinifer on the eighth day for the vaccination of Marie Françoise Rosnaro. She presented on August 20th on each arm six cicatrices, perfectly normal both in size and colour; no trace of adenitis, cervical or axillary, and no eruption upon the skin. In a word, this child was completely healthy, and in excellent general condition. Her mother, at the time of the visit, was in bed with rheumatic pains, but M. Depaul could find nothing suspicious about her, and her nipples were free from disease. The father of the child also was found free from all syphilitic infection.

This child Rosnaro was selected as vaccinifer for the next batch of vaccinations, of which more than eighty were made on the 3rd, 4th, and 5th of June. A list of forty-two children who

suffered from syphilis, having been vaccinated from Rosnaro, is given by M. Depaul ; they suffered, as usual, from primary ulcers at the seat of the vesicles, followed by various secondary affections. Two of these were used as vaccinifers, and their history is as follows :—Boulaire, a girl aged seventeen months, when seen in August, had fallen away a great deal, and had all the aspect of the syphilitic cachexia. There were two violet coloured cicatrices on the left arm, one upon a very manifestly indurated base, and three similar on the other arm ; indolent axillary adenitis on both sides, a little posterior cervical adenitis, and general roseola. The other, Anna Andran, aged nineteen months, was still suckled by her mother. There were three cicatrices of a violet tint, and reposing upon a somewhat indurated base on the left arm, and three on the right ; axillary indolent adenitis on both sides, considerable on the right, but less on the left ; posterior cervical adenitis, and general roseola.

From these children, Boulaire and Andran, a large number of vaccinations were done, certainly less, however, than from Rosnaro. M. Depaul, however, only succeeded in seeing and examining seventeen of them, and three of these seem to have had nothing amiss with them after the vaccination, but the rest exhibited phenomena similar to those observed in Boulaire and Andran.

Again, I must say that here is a series which has not received the amount of investigation which was required to establish the origin of the syphilis in a vaccine vesicle contaminated from the subject on which it existed. We start with the dry lymph from the prefecture, but we are told nothing about the child from whom it was taken. Was it impossible to have discovered this ? Then, the two children, Mahé and De Norcy, having been vaccinated, were ill ; probably they were infected with syphilis, but even this is not clear to my mind. Still, it does not follow that the lymph from the prefecture was in fault. There

was an intermediate agent, of whom we are told nothing but that she was a zealous vaccinator ; possibly she was *too* zealous, and not sufficiently careful. And, moreover, she was a mid-wife, and might have had syphilis herself, for anything we are told by M. Depaul, or have become in some way contaminated with the virus, or have got her clothing contaminated and have wiped her lancet upon it, or in some such way infected the children that she vaccinated. Who can tell? She is just as likely to have been in fault as the vaccine in any part of the series.

And then the odd thing is that Rosnaro, in the second generation from the dry lymph, did not suffer from syphilis at all, and yet, if we are to believe M. Depaul, communicated the disease from her vesicles to forty-two other children. How was this? If Mahé and De Norey received the syphilitic virus at all, and if Rosnaro was the link between them and the forty-two of the third generation, Rosnaro probably also received the virus into her arm ; and if so, why did she not suffer from syphilis? Was she already congenitally infected, or had she already had the disease fortuitously? We are told that she and her parents were healthy, only her mother kept her bed for rheumatismal pains, in which M. Depaul's experience could discover nothing syphilitic. Still, M. Depaul might have been wrong, and these pains might have been syphilitic, but we do not know that this is even probable. Here, then, is a break which we cannot get over. Mahé and De Norey doubtfully infected somehow. Rosnaro (second generation) not infected ; there is no real occasion why she should have been, for vaccinal virus does not always give syphilis when taken from a syphilitic subject, nor yet always when taken from a child into whose arm the virus of vaccinia and syphilis have both been deposited, and who subsequently suffers from both diseases. And then forty-two children infected (third generation), and from two of these

a number more. For my own part I must decline, until other facts may be forthcoming, to accept the inference that this is certainly a true series of vaccino-syphilitic inoculations, in the sense in which this term is used. No doubt vaccine and syphilitic virus were both deposited in the same puncture at the same time, wherever the two diseases appeared, but there is much doubt on my mind whether both came out of the same vesicle.

Before I proceed any further let us see how far we have got. Where, as in a host of instances recorded by Viennois and Rollet and other authors, as well as in those I have just quoted at length, the vaccine pustule is succeeded at the fall of the crust by a primary syphilitic sore, accompanied by indolent enlargement of the corresponding lymphatic glands, and followed by secondary constitutional symptoms, and especially where from these accidents other persons, mothers, brothers, sisters, &c., receive contagion, there can be no doubt in the world that, at the same puncture, at or about the same time, two separate viruses were deposited—the virus of the vaccine and the virus of syphilis; and both may take effect, one preceding the other in manifestation, in accordance with the vast difference in their periods of incubation. The thing has happened over and over again in cases which may now be counted by hundreds; so that this disposes for ever of the cry of “impossible.” *The one virus does not destroy the other; each produces its specific result.**

But is the syphilitic contamination of the puncture always fortuitous? Does it never happen that the vaccine virus and the syphilitic virus are each drawn at the same moment from the same vesicle, and that, too, a fine, perfect, complete, and unmistakable Jennerian vesicle? I believe that it does happen. I believe that it happened in the Rivalta and in the

* Five of the Rivalta infected children were revaccinated without result.

Lupara series; and this disposes, again, of the theoretical objection that the same vesicle cannot furnish both viruses, unless it be modified somehow in its characters. *The perfect character of the vesicle is no guarantee that it will not furnish both vaccine and syphilitic virus.*

But how? Out of the same "poche vaccinale," as Viennois calls it? Yes, unquestionably, to my mind; that is, *when the syphilitic virus has been previously placed there*, as it was in Manzone's instance, and as it was in every instance where syphilis was communicated from a vaccine vesicle before the constitution of the vaccinifer had been infected.

But what of those who, being constitutionally infected already, acted as vaccinifers? Is it true that the two viruses were *secreted* into the same pock without modifying its character, and then that the *pure* lymph taken from such a truly syphilitic child contained the two viruses mixed, and produced each its specific result on inoculation? Can we go so far as to admit this?

First, let us ask what we mean by "pure lymph." If a vaccine vesicle be lightly broken on the surface there issues a drop of limpid clear lymph; that is, if it be not opened after the areola is fully established. *This is pure lymph.* If the lymph be taken late, so that it is mixed with the products of common inflammation, it cannot be regarded as the pure secretion of the specific eruption. If a large number of children be vaccinated from the same pock, and it continues to give out fluid more in quantity than it can be believed to have at first contained, and this without itself lessening in bulk, the later lymph drawn cannot be regarded as "pure;" it is mixed with ordinary serous fluid from the blood. If the vesicle be ruptured roughly, or by repeated puncturing at greater and greater depths, blood is drawn and mixed with the lymph; this cannot, then, be called "pure" lymph.

Late lymph when opaque, serous lymph, and bloody lymph, are none of them "pure." They are mixed with blood or serum, or the products of inflammation, and are impure.

Now, the question is, whether, when lymph is "pure," it ever is capable of imparting syphilis when taken from a syphilitic child, or whether there is any reason to believe that it is only when "impure," in one of the ways I have mentioned, that it is capable of imparting the disease along with the vaccine. Recollect that I am now merely speaking of lymph drawn from a subject *infected at the time with constitutional syphilis*.

The view taken by MM. Viennois and Rollet is that the pure lymph from the vaccine vesicle in a syphilitic subject does *not* impart contagion, and that the lymph from such a subject only imparts syphilis when *mixed with some of the blood of the vaccinifer*. In fact, their view is that it is the blood accidentally drawn in taking the lymph, and not the contents of the vesicle itself, which is the infecting agent; and in this opinion they have now many supporters. Let us see, then, on what ground this view stands.

1. On the ground that it has been proved that *syphilitic blood will convey the disease by inoculation* upon a healthy person. I think the experiment of Professor Pelpizzari definitive as to this. The only difficulty in applying it is that in the successful inoculation of Dr. Bargioni a large quantity of blood was applied to a considerable surface, which is a very different thing from introducing a minute fraction of a drop, diffused through a quantity of diluting lymph. Still, I do not see that this is a difficulty absolutely insurmountable in our acceptance of the fact as it is applicable to vaccination. It would be opposed to all our ideas of the operation of a virus to allow it to influence our minds to this extent, inasmuch as we commonly, and, I believe, properly, hold that the *minutest quantity* of a virus, if its quality be

not impaired, is capable of producing the same specific effect as a large quantity when introduced into the system. The only conceivable way in which quantity of syphilitic blood can affect the inoculation would be by assuming that the virus is not equally diffused through the mass of blood. Thus, Rollet* says, "It is probable that the syphilitic virus is disseminated in the blood, and that, in order to inoculate successfully, we must either perform the inoculation with a quantity of liquid sufficiently large so as to multiply the chances that we may have of meeting with the virus in the inoculated mass; or else we must choose the blood which surrounds a syphilitic lesion, that is to say, take it at a point where there is some reason to presume that a sort of accumulation of the contagious principle would exist." If Rollet were correct here, his doctrine would be fatal to the hypothesis of which he and Viennois base upon the experiments hitherto performed.

2. On the ground that where the fact of admixture of blood with the lymph has been inquired into it has been found that the *syphilis has only been imparted* during vaccination in those cases *where blood has become mixed thus with the vaccine virus*. I am not quite satisfied, however, that this is so. Hübner positively denied to M. Viennois that any blood was drawn in his cases. It is true there is a suspicion that he was deceived about another point in the health of his vaccinifer (Keller), and might have been deceived here; but, at any rate, his positive assertion is worth as much as the evidence offered to us from M. Lecoq's *second* letter to M. Viennois about the soldiers that he vaccinated. In this letter he states that the two who were apparently syphilised were the *last two* of the batch who were vaccinated, and that he "recollected that, being at the end of the vaccine liquid, the lancet had drawn a little blood." Is it not a rule among all

* Op. cit., p. 349.

accurate observers to distrust all such distant recollections? And positively these are the only cases by the narration of which M. Viennois, in his first essay, seeks to establish his hypothesis. He quotes, indeed, a statement of Levrat (ainé), in 1848, that in cases of apparent transmission of syphilis blood was always drawn; but he does not submit the cases themselves to the judgment of his reader. In the vaccinations performed at Rivalta from Chiabrera, which were so disastrous in their result, there can be no doubt that blood was drawn; it was made a matter of complaint at the time. Again, the lymph used for the vaccinations of the children in the Lupara series is stated to have been tinged with blood. Still, up to now, I have recorded no evidence that the same event might not have followed, had the lymph used been free from blood. As to this point, however, we have *the crucial observations* of M. Sebastian, principal physician of the Maternity at Beziers. The event is thus recounted by M. Depaul, quoting from the 'Gazette des Hôpitaux':—

"On the 19th March, 1863, A. M— came to me with a child ten months old, who had been vaccinated eight days previously, to ask me to vaccinate from it the infants of two of her friends, who came with her. I proceeded to the operation, *taking the precaution not to make the pustules bleed*; they were well developed, and presented nothing abnormal. At the moment of collecting the vaccine, in order to make the *last puncture* upon the *second child*, the vaccinifer made a forcible movement, and, the point of the lancet penetrating more deeply, a little drop of blood came to colour the virus, which, to my regret now, was nevertheless inoculated. Twenty-two days after, this woman brought her child to me covered with an eruption. This is what I found. The vaccinal pustules were well developed and had passed through their course in a regular manner; there had been no

* 'De la Syphilis Vaccinale,' &c., p. 14.

exception to this except in respect of that which resulted from the *last puncture, the exact position of which I recollected perfectly well*. This pustule presented the characters of a veritable pseudo-chancere. It was surmounted by a crust, perfectly conical, of a dark colour, and very shiny; it was about two centimètres in diameter, and was slightly ulcerated at the circumference. Around it, for about half a centimètre, were lenticular, very smooth, regular papules, numerous and of a pale red colour. In the armpit of the same side was an enlarged gland of the size of an ordinary hazel nut. It was movable and painful to the touch. Forty-nine days later the pseudo-chancere was ulcerated, and presented a considerable induration. The child's body was covered with a syphilitic roseola, and some patches upon the genitals left no longer any doubts as to the nature of the affection. In order to make certain, I visited the child who had furnished the vaccine; he was very well, to all appearance, and the vaccinal pustules were completely healed. Inspection of the body, however, brought to view numerous spots of papular syphilitic eruption. The cervical glands were much engorged, and there were elevations on the genital organs and anus of a nature more than doubtful. The father of this child (the vaccinifer) told me that, being a soldier, he had an indurated chancre, for which he had been under treatment for thirty-five days at the hospital at Tours. He was far from being cured, and presented numerous traces of constitutional syphilis, such as crusts on the scalp, engorgement of the posterior cervical glands, syphilitic stains and patches on the anus. I ought to add that the other child, vaccinated with the same virus and at the same time, had had absolutely nothing the matter with it." But this is not the only occasion on which M. Sebastian vaccinated from syphilitic children. Some time after the event related, he vaccinated six children from such a vaccinifer, taking care to use the

vaccine virus alone, and *without any admixture of blood*. In these cases the only result was the vaccine vesicle. He even went so far as to vaccinate himself from such a child, and the vaccination succeeded, and from himself two children, and none of them had any symptoms of syphilis.

It is true this observation, as a *crucial* experiment, stands alone ; but it has, to my mind, a great convincing force. I cannot refuse, in the face of it, to believe that it *does* matter whether blood be drawn or not ; and it moreover throws light upon the numerous instances in which vaccinations have been performed by experienced operators from syphilitic children without producing infection. I have mentioned Heim as having vaccinated thus without ill effects ; so also have Husson, Bidart, Montain, Schreier, and Taupin. But such negative results, in the hands of men who would not willingly draw blood from a vesicle, can weigh nothing against the positive observation I have just narrated.

But I cannot leave the question here. M. Ricord *objects** that it is not credible that the blood should be contagious and not at the same time the secretions derived from it. I object to this sort of argument. It is not of a character to overcome a positive crucial observation. Besides, is it necessary that every product eliminated by a process of quasi-secretion from the blood shall contain everything that the blood itself contains ? If so, all we should have to do, when we needed to inoculate a virus, would be to raise a blister and inoculate the serum ; and, in vaccinating at first hand from the cow, we should obtain as good results from the secondary eruption surrounding the principal pustules on the teat as from the characteristic pustules themselves, which is not the case. Besides, we know that the ordinary secretions of a syphilitic subject—the saliva, tears, and urine, for example—do not contain the contagious principle. Yet the blood is inoculable.

* Discussion at the Académie—'De la Syphilis Vaccinale,' pp. 41 and 44.

Think, too, of what secretion is—a separation, not mechanical, but vital and selective in its character. Again, says M. Ricord, do vaccinifers and vaccinated children never bleed under the lancet of the operator? “Ah! here is a double danger. If the theory be true, you have then a double chance of meeting with the contagion—vaccinifers of imparting the disease to the vaccinated infants, into whom you insert their virus, and, by way of return, the latter to the vaccinifers, when you charge the instrument anew.” A happy thought. There is such a chance, let it be admitted. And such an event, although not traced, may yet have happened. The Lyons’ observers may thank M. Ricord for a new field of enquiry. Yet I cannot see how its truth or falsehood affects the question; especially I cannot see how it surmounts Sebastian’s observation.

But, M. Ricord says, if the blood be so important an element in the transmission of syphilis by vaccination, *how is it that so few children are syphilised?* An enormous quantity of lymph is yearly distributed by the Academy, and M. Ricord exhibited a drawing of the appearances in some lymph preserved for use there, which was swarming with blood-corpuscles; and no wonder, for M. Blot tells us that it was the custom to have the lymph taken by a man shaking with age, who could not fail in his ignorance and weakness sometimes to puncture the vesicles too deeply. One answer is, that probably *we do not hear of all the cases of syphilis* engendered by such lymph. The cases do not all come to light.

Another, and more satisfactory answer is, that *blood-inoculation of syphilis is not a very easy thing to effect*. I have already stated how often observers have failed in their experiments, and that, in the solitary case of success of Professor Pelpizzari, that a large quantity of blood was used. There seems to be necessary for success some conditions at present

little understood, both as respects the blood and the subject on whom it is inoculated. It is not improbable that the blood of a syphilitic subject does not always contain the virus in a condition to impart contagion. Perhaps the virus it does contain is more active when drawn from the vessels of a part in that state of irritation which must be present at the base of a vaccine vesicle, and that the greater the degree of the irritation the greater may be the activity of the virus. We must not forget Lee's observations about the auto-inoculability of chancre virus when the sore is greatly irritated. A third answer is that, after all, *hereditary syphilis* is not a thing daily met with—it is *much more rare than is commonly imagined*—so that it is not every sample of lymph, by a good many, that is at all likely to be drawn, even if bloody, from a syphilitic subject; especially when we consider that some selection is always made by somebody of the child from whom lymph is about to be taken, and that children who are manifestly out of health are not usually selected, whatever the cause of ill health may be. In common with hundreds of other medical men, I have seen bloody lymph employed in vaccination, and years ago have vaccinated with it myself, although I should not do so now, but never saw any bad result follow. M. Devergie* gives the following fact. There is, in Paris, an office for nurses, all from the lower classes of society, of whom 2200 are engaged yearly, and to whom a nurse child is given up, whom she takes into the country with her. They are visited from time to time by a medical man from the hospitals. M. Devergie inquired of M. Millard, who for eighteen months had been occupied in this service, how often he had met with syphilitic children during the period. His reply was, that the instances were so rare that he had not preserved any record of them, or thought it worth while to count them up. "I myself," says M. Devergie,

* Discussion at the Académie—'De la Syphilis Vaccinale,' p. 140.

“was physician in this service for three years, and if I have ever refused infants on this ground, it was at most once in a year or twice if you like to take it so.” The result of Devergie’s calculation is that, at the very extreme, the children of the class of “ouvriers” born in Paris constitutionally syphilitic do not exceed one in 170 births.

M. Depaul holds that vaccine virus, when *impure from the admixture of the serum of the blood* merely, if it be taken from a syphilitic child, may nevertheless be feared as capable of imparting syphilis. There can be no doubt that after a vesicle is exhausted, towards the last, if more virus be taken, it will be mixed more or less with ordinary serosity. But M. Depaul mentions no instance to prove that the virus, thus diluted, is more likely to confer syphilis than the lymph which first flows on pricking a vesicle. Still, there is this in his favour—such as the argument is worth—that it has commonly been observed, in cases of reputed syphilitic inoculation, that among a number vaccinated from the same child it has been those *last* vaccinated who have suffered, to the exclusion of those first vaccinated.* Viennois and others say probably because, the lymph being exhausted, deeper punctures have been made, and some blood drawn. But then there is an absence of proof that blood has *always* been drawn when the infection of syphilis has been conveyed with the vaccine. In M. Trousseau’s case there is no such proof, although he mentions it as a thing which *probably* happened. The young woman syphilised at the Hôtel Dieu was, however, the *last* of the batch vaccinated. I cannot say that I think M. Depaul’s hypothesis at all ridiculous; the exudation of

* It is to be observed, however, that in the Rivalta series some of the children *last* vaccinated from Chiabrera were not infected. Dr. Pacchiotti considers, and I think with good reason, that this may be due to a different vesicle having been opened for them to the bleeding one from which the other children were vaccinated.

serosity under irritation of the lancet, when lymph is sought at a great depth, is a very different affair to the secretion of lymph by the specific selection of the elements of a vaccine vesicle, although Depaul seems to confound the two things.* And however small the quantity of virus, the irritation of the vesicle might give it extraordinary energy. Still, under any circumstances, it is not yet proved that syphilitic virus passes with the serosity through the walls of the irritated vessels.

And so, too, with *impurity of lymph arising from the admixture of inflammatory products*. I thought once that the Rivalta inoculations might have been a cause of infection, in consequence of the lymph not having been taken before the tenth day; but it has since been shown that blood was drawn in abundance from Chiabrera. Still, again, it is a subject for inquiry and observation whether late lymph is thus more likely to impart syphilitic infection. Attention has now been fully drawn to the importance of blood contamination, and, in any future observations, the fact of its having been drawn or not is pretty sure to be recorded. We must wait and watch, if we would know whether this is the *only* impurity which will render dangerous vaccine lymph drawn from a syphilitic child.

I have thus endeavoured to review fairly and impartially the arguments adduced on either side of this important and most interesting controversy. For myself, I had no bias to commence with in either direction. I ask for facts, for truthful observations, and, where I find such as satisfy me as to their character, I take their evidence and weigh it to the best of my ability. It will have been seen that I spare criticism to neither party. Both sides have their enthusiastic and uncompromising advocates, who have, equally the one with

* Thus, he gives as a proof that such serum may contaminate lymph with syphilitic virus the fact that in "icterus neonatorum" the lymph is stained yellow.

the other, been led away, as I believe, by their zeal or the preconceived opinions which they held. I do not consider the subject even yet exhausted—we want more observations. Still, *there are certain points on which I think we are in a position to come to a distinct and definite conclusion.*

1. There are numerous cases on record to prove that the vaccine virus and the syphilitic virus may be introduced at the same spot by the same puncture of the vaccinating lancet.

2. From several instances on record, there can remain no reasonable doubt that the vaccine virus and the syphilitic virus may both be drawn at the same time, upon the same instrument, from one and the same vesicle.

3. The vesicle which is thus capable of furnishing both vaccine and syphilitic virus may present, prior to being opened, all the normal and fully developed characters of a true Jennerian vesicle, as ordinarily met with.

4. It is *not* satisfactorily established that, in *all* the instances recorded in which the vaccine and syphilitic viruses have both been introduced at the seat of puncture, and produced their specific effects, the syphilitic virus was derived from the interior of the vaccine vesicle.

5. There is reason to believe that the admixture of blood from the vessels of the syphilitic vaccinifer is a circumstance which increases very materially the chances of imparting syphilis to the child vaccinated with its lymph. I cannot go so far as to say that the admixture of such blood is the *only* condition under which infection can be imparted. I do not think that this is yet proved.

6. It appears that, in some instances, syphilis has been communicated by lymph taken from a vaccinifer upon whom syphilis had been inoculated with the vaccine, before he himself had become “infected”—indeed at a period before

even the primary sore had been formed. In such cases I cannot avoid believing that the vaccinifer was merely the depository of the syphilitic virus obtained from the child from whom he was himself vaccinated, or otherwise; the virus having at the most only undergone some local reproduction.

7. It is probable that even a weak syphilitic virus, when obtained from the interior of a vaccine vesicle, especially when irritated, may have acquired under such circumstances extraordinary energy.

8. The vaccine and syphilitic viruses exercise upon one another no mutually destructive influence; but, when introduced together into the same puncture, both may exercise their peculiar and specific operation—the one taking precedence of the other in accordance with the law of incubation proper to each.

9. After vaccino-syphilitic inoculation the first event is the development in the ordinary manner of the vaccine vesicle, which runs its course up to or nearly up to the fall of the crust. After or just prior to the fall of the crust a primary sore forms at its seat, the base becomes indurated after the manner of a Hunterian chancre, and the related lymphatic glands undergo an indolent enlargement. Subsequently, and mostly before the sore heals, symptoms of secondary or constitutional syphilis manifest themselves, and, under certain circumstances, a child thus infected may be a source of contagion to other individuals in the ordinary manner.

10. The cases recorded in which, after vaccination, a constitutional manifestation of syphilis takes place in the child, without the previous occurrence of a primary sore at the spot which the vaccine vesicle occupied, are none of them instances of vaccino-syphilitic inoculation. In these cases the child was either hereditarily syphilitic or had previously and fortuitously undergone the primary disease. The development

of the constitutional syphilis was the result, indeed, of the vaccination, but only in the sense that the disturbance of the system accompanying the vaccine disease promoted the operation of the virus pre-existent in the constitution. The most innocent vaccination under such circumstances may act thus.

11. Lastly, the infection of a child with syphilis in the process of vaccination, whether by true vaccino-syphilitic inoculation or fortuitously, is a rare event—*an extremely rare event*. All agree upon this.

Even M. Depaul, who in 1864 considered it his duty, as the Director of Vaccine in France, to represent the fact to his Government, and to impress upon the department charged with such matters the dangers to which children vaccinated from syphilitic vaccinifers were exposed, admits the extreme rarity of the occurrence. At the time he presented his report he admitted that, although two or three thousand children were annually vaccinated under his direction, he had not, up to that time, met with a single instance in which any one of them had been thus infected. MM. Bousquet and Steinbrenner added their testimony as never having witnessed such an event, notwithstanding an experience of vaccination scarcely to be equalled by any one. M. Trousseau stated, during the discussion in the French Academy, that in a period of sixty years not one single instance of transmission of syphilis to a child had been seen at the Academy, although more than 200,000 infants had been vaccinated there. And in Great Britain I must add that I do not know of one single well-authenticated, unquestionable instance on record*—*not*

* In making this assertion I am very likely to be met by the quotation of Dr. Whitehead's twenty-eighth case in his work 'On the Transmission from Parent to Offspring of some forms of Disease,' &c. (p. 174). This, however, is a case "*recorded from memory*." The period of the formation of the sores, the character of the sores, as described, the character of the axillary glandular

one—in which it has been established beyond dispute that syphilitic virus has been taken from a vaccine pock and inserted, with the vaccine, into the arm of a healthy child, imparting syphilis to him. Among all the correspondents of Mr. Simon there is not one which states he has seen such a case, except Mr. Hutchinson; and even he gives no such account of the one or two in which both a local and a constitutional affection followed as will alone permit us to accept them as true. Others talk about “suspicious,” “syphilitic taint” being communicated, and so on; but none give us the cases which, if incapable of being controverted, must have been noted down at the time, or have forcibly impressed their recollection, from their very rarity. On the other hand, the most experienced public and private vaccinators in this country deny most positively ever having met with such cases. Mr. Tomkins, of the National Vaccine Establishment, has never seen it, although during twenty-one years he has vaccinated over 40,000 children; nor yet Mr. Marson, whose experience is at least equal to his.

What, then, is this bugbear which has been raised to alarm fathers, mothers, and even physicians?—that has so frightened M. Depaul, that he has proposed to supplant arm-to-arm vaccination altogether by the adoption of animal vaccination? Is it such a very dangerous thing to vaccinate one child from another? Is there reason to regard the vaccinating lancet with such horror as to look upon every infant vaccinifer as a probable syphilitic? Surely not. After all, we may almost count upon our fingers the undoubted syphilitic vaccinifers who have, throughout all Europe, appeared at all probably to have communicated syphilis with their vaccine; and nearly all

swelling, and the nature of the disease said to have been conveyed to the nipples of the mother, are all such as to leave other explanations than syphilitic affection open for adoption. Nor is it at all satisfactorily shown that the vaccinifer was syphilitic.

of them have been in Italy. Not one—not one has been *proved* to have existed in England. I do not complain of the efforts made to introduce and to foster animal vaccination. I approve of them, but not because I fear syphilitic infection in the vaccination of one child from another.

Still, science has her *mission*—to investigate, to interrogate nature, and fearlessly and boldly to declare the truth; to suppress nothing, to magnify nothing, to distort nothing. *Medicine also has her duties*—to accept the teachings of science, and to gather from them rules of practice; to reject nothing that is proved to be true, because she does not comprehend and cannot explain it; to give to every truth, however limited its application, all the consideration to which it is entitled. More than this. Medicine, as a practical art, is bound to govern herself by laws which science is not to be controlled by. She does not and ought not to wait for absolute demonstration; she allays the unreasoning fears of the timid, but, at the same time, gives them the advantage of her caution.*

The following practical inferences may be drawn from the discussion :

1. Unless exceptionally, lymph for vaccination ought not to be taken from an adult person, inasmuch as, few as the chances are, under any circumstances, of meeting with a syphilitic vaccinifer, they are greater in the instance of adults than of children.

* “In medicine and all but the demonstrative sciences there is often light enough to guide our conduct when there is not enough to gratify our curiosity; hence, practical men are often compelled to act on evidence which would sound unsatisfactory in the statement. There is no paradox in saying, that he who can give a striking reason for every measure which he adopts is for that very reason a bad medical adviser, because he must neglect many which are necessary and useful, but the reasons for which at the outset are extremely obscure.”—Gooch’s Works, Sydenham Society edition, p. 195.

2. It is prudent not to take lymph from an infant under three months of age, since, if any syphilitic taint exist, it will mostly have declared itself by this time.

3. It is prudent, before taking lymph, to make a careful examination of the surface of the body of the vaccinifer, of the mouth, genitals, and anus, and to reject any infant manifestly out of health, or presenting any eruption or morbid appearances anywhere.

4. According to the researches of Mr. Hutchinson,* what is called "strumous corneitis" or "interstitial keratitis," and iritis in infants and young children, are evidences of hereditary syphilis; and, after second dentition, evidence of constitutional syphilis is afforded by a peculiar malformation of the central upper incisor teeth. Any child presenting these conditions, or any other appearances indicative of syphilis, should be rejected.

5. Inquiry should be made into the health of the parents, that of the mother being most important, and also of the nurse. At any rate, they should be seen; and the least suspicion of syphilis in either, or in the child itself, should lead to the rejection of the vaccinifer. Happily the rarity of constitutional syphilis lessens the necessity for a full and complete examination, except under circumstances of suspicion, and so the practical difficulty of following out this rule.

6. The lancet used in vaccination should be strictly reserved for this purpose alone, and at the time of operation should, together with the hands of the operator, be scrupulously clean and pure. When vaccinating a batch of children from one vaccinifer the lancet should be wiped on clean lint after completing the operation upon each. There can be no excuse for neglecting so simple and easy a precaution as this.†

* 'A Clinical Memoir on Certain Diseases of the Eye and Ear, consequent on Inherited Syphilis.'

† Some persons recommend the use of a *needle* in vaccination in preference

7. In taking lymph from the vaccinifer, great caution should be observed in opening the vesicle superficially, so as not to draw any blood. Should blood be accidentally drawn, the vesicle should be abandoned, the lancet cleaned, and a new vesicle opened.

8. It is prudent to abstain from vaccinating more children from one vesicle than can be conveniently done with the lymph supplied from the first punctures made into its surface.

9. It is prudent (and a practice to be recommended for other reasons also) to abstain from the use of lymph taken from a vesicle after the areola is established, or from one irritated or damaged.

10. Parents and nurses should be warned against the common and nasty practice of moistening the punctured spots or vesicles, when they arise, with saliva, under the pretence of allaying local irritation, and also against applying old linen to the spots unless previously well washed. Nothing but soft lint should be applied to protect the young vesicles.

11. No child should be vaccinated whose parents or nurse are known at the time to be the subject of any primary or secondary manifestations of syphilitic disease, unless removed temporarily from the influence of their contagion.

Now, it cannot be said that every one of these precautions is not *practicable*; and I firmly believe that their observance would entirely suffice to prevent the most remote chance of syphilitic infection, either in the act of vaccination or subsequently during the progress of the vaccine disease. They

to the lancet, partly because it is less likely to be contaminated, and partly because, by using less lymph, the chances of introducing blood with it are lessened.

are mostly such as commend themselves to the adoption of medical men, quite independently of the dread of performing a vaccino-syphilitic inoculation. Let medical men retain always uppermost in their minds a due sense of the high responsibility attaching to every act they perform, to every word they speak, to every word they abstain from saying, to every act they abstain from doing. I believe that, with very few exceptions, this responsibility is felt, and that it does direct the conduct of the medical profession in all matters of professional importance. And what I want to impress upon all, surgeons and the public, is that the act of vaccination is an important act—one that is to be undertaken deliberately—one to be performed seriously, carefully, neatly, and cleanly. All that relates to the vaccinifer, his age, his health, the character of his pock, and the cleanliness of his surface, should be considered; all that relates to season and temperature as elements of success; all that relates to the operation itself, the instrument employed, the opening of the vesicle, the appearance of the lymph which flows; all that relates to the state of health of the candidate for vaccination, and to the protection and management of the nascent vesicles; all should be taken into account when the operation of vaccination is contemplated. Medical men and parents alike should drive from their minds the idea so prevalent, that vaccination is but a trivial operation at the most, which may be performed any how, at any time, with dry lymph or preserved lymph equally well as from arm to arm, from vesicles at any stage so long as fluid can be got from them, and from any person or any child that has a vesicle on his arm to furnish it. They should keep in mind that in the act of vaccination they are not merely imparting a protection, not merely performing a sort of magic rite, but that they are engaged, in very truth, *in implanting the seeds of a DISEASE*. They should keep in mind the *object* of the act,

namely, that the vaccinated individual shall undergo this disease fully, completely, and with the utmost perfection attainable. God forbid that the silly reasoning of illogical detractors, the exaggerated fears of the timid, or the subtle insinuations of selfish quackery, should ever lead the public mind of this nation to question the value of the grand discovery of our own countryman! God forbid that we should ever underestimate, or regard with suspicion and distrust, perhaps the highest boon which a merciful and loving Creator has ever permitted one single man to bestow upon his fellows!

“Is the enemy at our gate? Is it syphilis that is there, threatening to invade our hearths under the guise of vaccine? No; you know that it is not. It is not syphilis, it is small-pox which is at our gate.”* Let this word of the veteran of the French Academy ring perpetually in our ears. Man, indeed, is the agent for imparting to man this inestimable protection—man, weak, fallible, purblind, imperfect. Like himself, his inventions are imperfect. Man, indeed, ever gropes out his way amid the difficulties of life slowly, painfully, falteringly. He makes many a false step; often is he deluded by a shadow, or alarmed by a phantom without substance. The “light of his intellect”† is indeed a dull and feeble light, and does not always burn with equal brightness; but, feeble as it is, it is all that is permitted him to use. Thank God, the more he uses it the more diligently he trims it; the brighter and clearer his path becomes, the more he sees of the road he has to follow; the more firmly and confidently he plants his feet, the more certainly and speedily he attains his end.

* Ricord, Discussion in the French Academy, January 10th, 1865.

† “The light of the understanding is not a dry and pure light, but is drenched in the will and affections, and the intellect forms the sciences accordingly; for what men most desire should be true they are most inclined to believe. The understanding, therefore, rejects things difficult, as being

SUMMARY.

The following is a summary of the more important inferences I draw from the preceding discussion :

1. That the practice of vaccination is worthy of public confidence as a protection against attacks of smallpox, and that it is (*when satisfactorily carried out*) as complete a protection now as it was at the earlier part of the present century.

2. That the practice of variolous inoculation, which preceded it in order of time, also served as a protection against subsequent casual contagion ; but, as it never could be made as general as that of vaccination, and as, when in use, it had the disadvantage of multiplying the foci of contagion and so of increasing the ravages of smallpox among the unprotected, a recurrence to the practice is to be deprecated.

3. That there is no reason to believe that the practice of vaccination, while lessening the ravages of smallpox, has had the effect of promoting the occurrence of other fatal maladies.

4. That vaccination in itself is, as a general rule, a harmless operation, which may be performed upon the youngest infants without extraordinary danger. Now and then, indeed, it has been followed by serious and even fatal results ; but, to admit this, is to say no more than that, as a human inven-

impatient of inquiry, things just and solid, because they limit hope, and the deeper mysteries of nature through superstition ; it rejects the light of experience through pride and haughtiness, as disdaining the mind should be meanly and waverily employed ; it excludes paradoxes for fear of the vulgar. And thus the affections tinge and infect the understanding numberless ways, and sometimes imperceptibly."—Bacon, 'Nov. Org.,' pt. i, sect. ii, aph. 12.

"The kingdom of man, which is founded in the sciences, can scarcely be entered otherwise than the kingdom of God, that is, in the condition of little children."—Ibid., sect. iii, aph. 68.

tion, it is not absolutely perfect or free from the casualties which sometimes attend the most trifling surgical accidents and operations, or the administration of an ordinary dose of medicine.

5. That the protection afforded against smallpox by vaccination is neither unconditional nor constantly unlimited, but that many of the conditions upon which it depends are under the control of mankind.

6. That, for the majority of persons vaccinated in infancy, and not unusually exposed to the contagion of smallpox, vaccination serves as a life-long protection against attacks of the disease.

7. That daily experience demonstrates that vaccination in infancy is not an absolute protection to *all* persons against a future attack of smallpox; and, moreover, that in some persons smallpox has happened within a very short period of time after vaccination.

8. That neither is smallpox an absolute protection to *all* persons against a recurrence of the disease. Still, the recurrence of smallpox is a much more rare event than smallpox after vaccination. The vaccine disease produces a less deep impression upon the system than smallpox, for which, indeed, it is but the pathological "*substitute*."

9. That post-vaccinal smallpox is most commonly met with in epidemic seasons, or under circumstances of unusual and prolonged exposure to contagion.

10. That the protection afforded by vaccination is more complete the more complete the development of the vaccine disease, and especially the more marked the impression made by it upon the system. With the lymph currently in use a larger number of vesicles are more protective than a smaller number.

11. That where vaccination fails to impart absolute protection against smallpox contagion, it nevertheless modifies, in

the majority of instances, the course of the disease, and renders it less fatal. In this view, a person with only a single vaccine scar upon his arm, and that of an indifferent character, is in a much better position than a person wholly unvaccinated.

12. That the protection against the contagion of smallpox commences from the time that the areola is formed around the vaccine vesicle.

13. That a large number of persons, vaccinated in infancy, reacquire, in the progress of years, the power of developing the virus of smallpox in their system, and hence that, in epidemic seasons or under circumstances of unusual or prolonged exposure, such persons are endangered; but their danger is lessened by the fact that their readiness to receive the contagion into their system does not keep pace with the return of capacity for developing the virus.

14. That the weakening of the protective power of infant vaccination thus implied is progressive, and that it occurs most rapidly during the period of most active bodily growth, namely, from infancy to puberty. In addition to this, there is a period of life, namely, about the age of fifteen to about that of twenty-five years, during which both vaccinated and unvaccinated (although unequally) are especially liable to take smallpox on exposure to contagion.

15. That, notwithstanding this loss of protection against contagion, there does not occur in the vaccinated a corresponding lessening of protection against the fatality of smallpox. The fatality of post-vaccinal smallpox is determined chiefly by the age of the person attacked, after a similar manner to the fatality of smallpox in the unvaccinated. At any distance of time, however, from infant vaccination, smallpox is a less fatal disease than it is in the unvaccinated.

16. That the return of receptivity for smallpox contagion

(more or less complete) is a process in accordance with the general laws of physiology, and so does not detract from the value of vaccination as a human invention.

17. That the amount of protection conferred by vaccination being dependent upon the perfection of the vaccine disease, it follows that whatever tends to detract from such perfection favours at the same time the return of receptivity for smallpox contagion.

18. That, although there is some reason to think that vaccination in *very* early infancy may impress the constitution less than when the operation is deferred, and, therefore, that under special conditions and modes of life there may be an advantage in deferring the operation; yet that, considering how prone children in the first years of life are to suffer from smallpox, and how fatal a disease it is in such young subjects, and considering also the practical difficulties in the way of effectual vaccination of children of any age in large communities, it would be imprudent and unwise to defer vaccination in populous places or in any but very isolated communities beyond the first few months after birth.

19. That the production of a perfect or imperfect vaccine disease, and so, in a degree, the amount of protection imparted in vaccination, depends to a considerable extent upon the adoption or neglect of certain precautions, of universal applicability, which every vaccinator is bound to regard, and having reference to the source from which the lymph is derived, to the subject vaccinated, and the circumstances and mode in which the operation is performed.

20. That there is very good reason to believe that the vaccine disease is less perfect, and hence probably less protective against smallpox, when produced by the inoculation of virus which has already undergone a large number of human transmissions. This conclusion implies, further, that probably post-vaccinal smallpox would be a more rare disease were

lymph of an earlier human generation (counting from the cow) more commonly in use.

21. That there is reason to believe that *sometimes* the occurrence of post-vaccinal smallpox is favoured by a constitutional peculiarity of an individual, of such a character as to promote the unusually quick return of receptivity for the contagion.

22. That there is good ground for recommending that from time to time a recurrence to the original source of vaccine lymph should be had, and for considering that under some national arrangements it would be quite practicable to place such a supply of original lymph at the command of practitioners, either derived from animals naturally suffering from the disease, or by the adoption of a system of inoculation of cow-pox matter from heifer to heifer in repeated and continuous succession.

23. That, considering the progressive loss of protection imparted by infant vaccination in a proportion of vaccinated persons, and the impossibility of distinguishing between those in whom it has and those in whom it has not occurred, and also considering that a large number of persons are more or less endangered by the return of capability for developing the smallpox virus, and considering further the special liability to smallpox during the years immediately following the establishment of puberty, *revaccination* is to be strongly recommended for *all* persons at the age of about sixteen years. Such persons so vaccinated may be regarded as permanently protected, and there is no occasion for any further repetition of the process.

24. That there is no reason to believe that, with the use of ordinary care in vaccinating, smallpox is ever likely to be communicated in the act of vaccination.

25. That there is no reason to believe that scrofula or ordinary skin-diseases are communicable by vaccination, but that a latent tendency to constitutional disease may be stimu-

lated into activity by the systemic disturbance accompanying the evolution of vaccinia.

26. That constitutional syphilis is no exception to this conclusion ; for that, in children hereditarily infected with syphilis, vaccination is apt to hasten the manifestation of the *already existent*, though *latent, constitutional* taint.

27. That numerous cases are on record to prove that the vaccine virus and the syphilitic virus may both be introduced at the same spot and even by the same puncture of the vaccinating lancet, and that, in such instances, both viruses may take effect, the vaccine vesicle running naturally through its several stages, and being succeeded by a chancre on the fall of the crust.

28. That it is *not* established satisfactorily that, in *all* these recorded cases, the virus of the vaccine disease and the virus of syphilis were both taken by the vaccinator out of the vaccine vesicle.

29. That there are cases on record, however, in which it is difficult to arrive at any other conclusion than that the virus of the vaccine and the virus of syphilis were both taken by the vaccinator out of a vaccine vesicle, which presented at the time all the normal characters of a perfect vaccine pock.

30. That this accident may happen when the vaccinifer is constitutionally infected with syphilis, but it may also happen prior to the development of the chancre in a vaccinifer himself previously untainted, but who had been vaccinated with lymph contaminated with syphilitic virus.

31. That in the case of a vaccinifer constitutionally infected with syphilis, the danger of imparting syphilis with the vaccine is greatest where some of the blood of the vaccinifer has become accidentally mingled with the lymph obtained in puncturing the vesicle.

32. That in vaccinating from a syphilitic vaccinifer,

syphilis is *not necessarily* imparted to the person vaccinated.

33. That although it cannot be denied that such a danger exists, it is one the practical bearing of which has been very greatly exaggerated—it is a danger practically insignificant, and there is no reason whatever to believe that it exists to such an extent as to detract from the value of arm to arm vaccination, as a popular practice of general applicability.

34. That even were the danger greater than it actually is, the adoption of a few simple precautions on the part of the vaccinator would render the inoculation of syphilis in vaccination almost an impossible event.

A P P E N D I X.

TABLE I.

Showing the comparative mortality from smallpox in London, in decennial periods before the discovery of inoculation, during the practice of inoculation, and under vaccination.

[From Parliamentary Return, 26th April, 1853.]

Periods.	Mean annual smallpox deaths per 1000 deaths.	Comparative numbers.
1650—1660 . . .	48	} . . 56 . . No protection.
1660—1670 . . .	36	
1670—1680 . . .	71	
1680—1690 . . .	74	
1690—1700 . . .	51	
1750—1760 . . .	100	} . . 96 . . Inoculation.
1760—1770 . . .	108	
1770—1780 . . .	98	
1780—1790 . . .	87	
1790—1800 . . .	88	
1810—1820 . . .	42	} . . 25 . . Vaccination.
1820—1830 . . .	32	
1830—1840 . . .	23	
1840—1850 . . .	18	
*1850—1860 . . .	12†	

During the first five years of the present decenniad, that is, from 1861 to 1865 inclusive, the ratio of smallpox deaths to deaths from all causes has been $10\frac{1}{2}\dagger$ per 1000, notwithstanding that the epidemic year, 1863, is included.

* I have added this from Registrar-General's returns. The comparative number, therefore, is not the same as in the original table; but as five decenniards are embraced in the vaccination period the result is more satisfactory.

† Medical officers of health were first appointed in London, in 1855, and have all made it their business to give information to public vaccinators of outbreaks of smallpox in their districts.

TABLE II.

Showing the difference between the mortality from smallpox in England, Scotland, and Ireland during a period in which there was no law compelling vaccination, and that in countries where vaccination is more or less directly compulsory.

[From Parliamentary Return, April 26th, 1853.]

1.

Table showing the mortality in various places in England, Scotland, and Ireland from smallpox, as compared with the total mortality for 10 years, ending 1850—1851.

Deaths from smallpox.						per 1000 deaths from all causes.		
London	16·0		
Birmingham	16·6	”	”
Leeds	17·5	”	”
Paisley	18·0	”	”
Edinburgh	19·4	”	”
Liverpool	21·0	”	”
Dundee	24·2	”	”
Perth	25·0	”	”
Greenock	34·6	”	”
Glasgow	36·0	”	”
Dublin	25·66	”	”
Cork	39·5	”	”
Galway	35·0	”	”
Limerick	41·0	”	”
Connaught (10 years, ending 1841)	60·0	”	”
All Ireland (ditto)	49·0	”	”
England and Wales (8 years)	21·9	”	”

2.

Table showing the mortality from smallpox in various countries in which vaccination is directly or indirectly compelled, as compared with the total mortality.

	Deaths from smallpox.			per 1000 deaths from all causes.		
Westphalia	6.0					
Saxony	8.33	”	”	”	”	”
Rhenish Provinces	3.75	”	”	”	”	”
Prussian Silesia	5.25	”	”	”	”	”
Pomerania	7.75	”	”	”	”	”
All Prussia	7.5	”	”	”	”	”
Lower Austria	6.0	”	”	”	”	”
Trieste	5.15	”	”	”	”	”
Bohemia	2.0	”	”	”	”	”
Lombardy	2.0	”	”	”	”	”
Venice	2.2	”	”	”	”	”
Sweden	2.7	”	”	”	”	”
Bavaria	4.0	”	”	”	”	”

TABLE III.

Abstract of Table in Mr. Simon's Report ('Papers Relating to the History and Practice of Vaccination'), p. 171, showing the mortality from smallpox in Copenhagen for a period of 100 years.

Years.	Popula- tion.	Died of small- pox.	Remarks.	Years.	Popula- tion.	Died of small- pox.	Remarks.
1750	60,000	1457		1802	...	73	Vaccination estab- lishment erected. Commission recog- nise protective power of vacci- nation.
1751	...	80					
1752	...	113		1803	...	5	
1753	...	53	Measles prevalent.				
1754	...	9	Inoculation intro- duced.				
1755	...	1117	Inoculation hospi- tals founded.	1804	...	13	Bombardment of Copenhagen by the English.
1756	...	125		1805	...	5	
1757	...	13	Measles prevalent.	1806	...	5	
1758	...	13	Ditto.	1807	...	2	
1759	...	1079	Epidemic of scarla- tina.				
1760	...	118	Inoculation hospi- tals closed.	1808	...	46	
				1809	...	5	

TABLE III—*continued.*

Years.	Popula- tion.	Died of small- pox.	Remarks.	Years.	Popula- tion.	Died of small- pox.	Remarks.
1761	...	4	} Measles and ty- phoid prevalent.	1810	...	4	Decree ordering vaccination pro- mulgated.
1762	...	7					
1763	...	167					
1764	...	480		1811	100,975	...	
1765	...	138		1812	
1766	...	42		1813	
1767	...	6		1814	
1768	...	27		1815	
1769	70,495	1219		1816	
1770	...	22	Inoculation estab- lishment outside city.	1817	Vaccination placed under control of Board of Health.
1771	...	8		1818	
1772	...	22		1819	
1773	...	190		1820	
1774	...	116		1821	
1775	...	276		1822	
1776	...	86		1823	
1777	...	7	} Scarlatina very prevalent.	1824	...	41	
1778	...	270		1825	...	12	
1779	...	283					
1780	...	98		1826	...	29	
1781	...	174	Measles prevalent.	1827	...	4	
1782	...	332		1828	...	1	
1783	...	123	Inoculation esta- blishment closed.	1829	...	29	
1784	...	77		1830	...	3	
1785	...	427		1831	
1786	...	193		1832	...	3	
1787	...	136	Scarlatina preva- lent.	1833	...	19	} Revaccination be- came general in May month.
				1834	119,292	26	
1788	...	185		1835	...	434	} Revaccination or- dered for the army.
1789	...	323		1836	119,591	81	
1790	...	140					Revaccination or- dered for the navy.
1791	...	297	Measles prevalent.	1837	...	1	
1792	...	155		1838	...	2	
1793	70,495	139		1839	
1794	...	452		1840	...	2	
1795	...	248	Scarlatina preva- lent.	1841	
				1842	...	35	
1796	83,604	357		1843	...	111	
1797	...	423		1844	...	83	
1798	...	386		1845	126,787	7	
1799	...	45		1846	
1800	...	35	Scarlatina preva- lent.	1847	
				1848	...	2	
				1849	...	7	
1801	91,631	486	VACCINATION first introduced.	1850	129,695	...	

TABLE IV.

Showing for several countries the death rate from smallpox during a period prior to the introduction of vaccination and since the introduction of vaccination.

[From 'Papers Relating to the History and Practice of Vaccination,' 1857, p. xxiii.]

Terms of years respecting which particulars are given.	Territory.	Approximate average annual death rate from smallpox per million of living population.	
		Before introduction of vaccination.	After introduction of vaccination.
1777—1806 and 1807—1850	Lower Austria . . .	2,484	340
1777—1806 „ 1807—1850	Upper Austria and Salzburg . . .	1,421	501
1777—1806 „ 1807—1850	Styria . . .	1,052	446
1777—1806 „ 1807—1850	Illyria . . .	518	244
1777—1806 „ 1838—1850	Trieste . . .	14,046	182
1777—1803 „ 1807—1850	Tyrol and Voralberg . .	911	170
1777—1806 „ 1807—1850	Bohemia . . .	2,174	215
1777—1806 „ 1807—1850	Moravia . . .	5,402	255
1777—1806 „ 1807—1850	Austrian Silesia . . .	5,812	198
1777—1806 „ 1807—1850	Gallicia . . .	1,194	676
1787—1806 „ 1807—1850	Bukowina . . .	3,527	516
	1817—1850 Dalmatia	86
	1817—1850 Lombardy	87
	1817—1850 Venice	70
	1831—1850 Military Frontier	288
1776—1780 and 1810—1850	Prussia (Eastern Provinces) . . .	3,321	556
1780 „ 1810—1850	Prussia (Western ditto)	2,272	356
1780 „ 1816—1850	Posen . . .	1,911	743
1776—1780 „ 1810—1850	Brandenburgh . . .	2,181	181
1776—1780 „ 1816—1850	Westphalia . . .	2,643	114
1776—1780 „ 1816—1850	Rhenish Provinces . .	908	90
1781—1805 „ 1810—1850	Berlin . . .	3,422	176
1776—1780 „ 1816—1850	Prussian Saxony . . .	719	170
1780 „ 1810—1850	Pomerania . . .	1,774	130
	1810—1850 Prussian Silesia	310
1774—1801 and 1810—1850	Sweden . . .	2,050	158
1751—1800 „ 1801—1850	Copenhagen . . .	3,128	286

TABLE V.

Showing the proportion of deaths from smallpox in Prague during 7 years prior to the introduction of vaccination, and 24 years subsequent to the introduction of vaccination.

[Abstract of Tables in 'Papers Relating to the History and Practice of Vaccination,' p. 162.] .

	Years 1796—1802.	Years 1832—1855.
Proportion to population of deaths generally } .	1 : 32 . . .	1 : 32 $\frac{1}{3}$.
Deaths from smallpox to population } .	1 : 396 $\frac{2}{3}$. . .	1 : 14,741 $\frac{1}{3}$.
Deaths from smallpox to total number of deaths } .	1 : 12 $\frac{1}{3}$. . .	1 : 457 $\frac{2}{3}$.

TABLE VI.

Showing the annual mortality from smallpox in England and in London at three periods, viz.: 1. Before the enactment of any vaccination laws; 2. After vaccination was provided gratuitously, but was not made obligatory; and, 3. Since vaccination has been made obligatory by statute.

1. Deaths from smallpox in England.

1. Before the enactment of any vaccination laws.		2. Vaccination provided gratuitously but not made obligatory.		3. Vaccination made obligatory.	
Years.	Deaths.	Years.	Deaths.	Years.	Deaths.
1838	16,268	1841	6,368	1854	2,808
1839	9,131	1842	2,715	1855	2,525
1840	10,434	*1847	4,226	1856	2,277
		1848	6,903	1857	3,936
		1849	4,645	1858	6,460
		1850	4,666	1859	3,848
		1851	6,997	1860	2,749
		1852	7,320	1861	1,320
		1853	3,151	1862	1,628
				1863	5,964
Average annual deaths } 11,944		Average annual deaths } 5,221		Average annual deaths } 3,351	

* Deaths from 1843 to 1846 inclusive not given in 'Registrar-General's Reports.'

TABLE VI—*continued.*2. *Deaths from smallpox in London.*

1. Before the enactment of any vaccination laws.		2. Vaccination provided gratuitously, but not made obligatory.		3. Vaccination made obligatory.	
Years.	Deaths.	Years.	Deaths.	Years.	Deaths.
1838	3,817	1841	1,053	1854	694
1839	634	1842	360	1855	1,033
1840	1,235	1843	438	1856	531
		1844	1,804	1857	156
		1845	909	1858	242
		1846	257	1859	1,158
		1847	955	1860	898
		1848	1,617	1861	217
		1849	518	1862	345
		1850	498	1863	2,012
		1851	1,066	1864	537
		1852	1,159	1865	646
		1853	211	1866	1,388
Average annual deaths } 1,859		Average annual deaths } 826		Average annual deaths } 758	

TABLE VII.

Showing the annual mortality to 1000 persons living in Sweden at four several periods during the last and the present century.

[Simon's 'Papers Relating to the History, &c., of Vaccination,' p. 51.]

Ages.	21 years, 1755—1775.	20 years, 1776—1795.	20 years, 1821—1840.	10 years, 1841—1850.
0—5	90·1	85·0	64·3	56·9
5—10	14·2	13·6	7·6	7·8
10—15	6·6	6·2	7·4	4·4
15—20	7·6	7·0	4·9	4·8
20—30	9·2	8·9	7·8	6·8
30—40	12·2	11·6	11·8	9·8
40—50	17·4	16·1	16·7	14·5
50—60	26·4	23·9	26·0	23·6
60—70	48·1	49·3	49·4	46·3
70—80	102·3	104·1	112·9	102·8
80—90	207·8	197·4	243·7	228·5
90 and upwards	394·1	351·3	396·4	375·8
All ages	28·9	26·8	23·3	20·5

The large figures in the fourth and fifth columns relate to that section of the population which has been born since the introduction of vaccination, and of which (persons under 30 in the fourth and under 40 in the fifth column) the greatest part is undoubtedly vaccinated. Of persons ten years older, especially in the last column, many are vaccinated; of persons still older a diminished and diminishing proportion.

TABLE VIII.

Abstract of Mr. Marson's Table, showing the influence of good or bad vaccination, as evidenced by the number and character of the cicatrices, upon the severity and fatality of smallpox.

	Number of patients.		Cases.	Small-pox unmodified.	Small-pox modified.	Rate per cent. of mortality after deducting cases affected by super-added disease.
1. Having <i>one</i> vaccine cicatrix	1357	good	768	209	559	4.23
		indifferent	589	251	338	11.95
2. Having <i>two</i> ditto	888	good	608	122	486	2.68
		indifferent	280	91	189	7.29
3. Having <i>three</i> do.	274	good	187	31	156	1.63
		indifferent	87	19	68	2.32
4. Having <i>four</i> or more ditto	268	good	202	20	182	0.99
		indifferent	66	6	60	0.00
5. Stated to have been vaccinated, but having no cicatrix	290	...	290	187	103	21.73
6. Stated to have been vaccinated, but particulars of cicatrix not recorded		...	17	9	8	6.66
Total . . .	3094		3094	945	2149	6.76

* In quoting Marson's Table, Mr. Simon states that further particulars forwarded to him by the author make the fatality of smallpox in persons with four or more cicatrices rather less than *half of one per cent.*

TABLE IX.

Showing the relative fatality of smallpox when it affects the unvaccinated and the vaccinated.

[‘Papers Relating to the History and Practice of Vaccination,’ p. xxvii.]

Places and times of observation.	Total Number of cases observed.	Death rate per 100 cases.	
		Among the unprotected.	Among the vaccinated.
France, 1816-41	16,397	$16\frac{1}{3}$	1
Quebec, 1819-20	?	27	$12\frac{2}{3}$
Philadelphia, 1825	140	60	0
Canton Vaud, 1825-9	5,838	24	$2\frac{1}{6}$
Darkehmen, 1828-9	134	$18\frac{1}{2}$	0
Verona, 1828-39	909	$46\frac{2}{3}$	$5\frac{2}{3}$
Milan, 1830-51	10,240	$38\frac{1}{3}$	$7\frac{2}{3}$
Breslau, 1831-33	220	$53\frac{1}{2}$	$2\frac{1}{9}$
Wurtemberg, 1831 $\frac{1}{2}$ -5 $\frac{1}{2}$	1,442	$27\frac{1}{3}$	$7\frac{1}{10}$
Carniola, 1834-5	442	$16\frac{1}{4}$	$4\frac{2}{5}$
Vienna Hospital, 1834	360	$51\frac{1}{4}$	$12\frac{1}{2}$
Carinthia, 1834-5	1,626	$14\frac{1}{2}$	$\frac{1}{2}$
Adriatic, 1835	1,002	$15\frac{1}{2}$	$2\frac{1}{3}$
Lower Austria, 1835	2,287	$25\frac{1}{3}$	$11\frac{1}{2}$
Bohemia, 1835-55	15,640	$29\frac{1}{5}$	5
Gallicia, 1836	1,059	$23\frac{1}{2}$	$5\frac{1}{7}$
Dalmatia, 1836	723	$19\frac{1}{3}$	$8\frac{1}{4}$
London Smallpox Hospital, 1836-56	9,000	35	7
Vienna Hospital, 1837-56	6,213	30	5
Kiel, 1852-3	218	32	6
Wurtemberg, no date	6,258	$38\frac{9}{10}$	$3\frac{1}{2}$
Malta, no date	7,570	$21\frac{7}{100}$	$4\frac{2}{10}$
Epidemiological Society returns, no date	4,624	$19\frac{1}{10}$	$2\frac{1}{10}$

TABLE X.

Abstract of Mr. Marson's Table ('Med. Chir. Trans.,' vol. xxxvi, p. 369), showing the fatality of smallpox in the vaccinated and unvaccinated admitted into smallpox hospital, from 1836—1851 inclusive.

	Ratio per cent. of mortality after deducting cases affected by superadded disease.
Unprotected	35·55
After inoculated smallpox	23·07
After vaccination with cicatrix or cicatrices	5·25
Stated to have been vaccinated, but having no cicatrix.	21·73

TABLE XI.

*Showing the results of revaccination of recruits in Württemberg Army, 1836, according to the number and character of the primary cicatrices.**

No. of cicatrices.	Number of persons.	Success of revaccination generally.						Number of persons.	Success with good cicatrices.						Number of persons.	Success with bad cicatrices.					
		Com-plete.	Pr. ct.	Modi-fied.	Per ct.	Failure.	Per ct.		Com-plete.	Pr. ct.	Modi-fied.	Per ct.	Failure.	Per ct.		Com-plete.	Pr. ct.	Modi-fied.	Per ct.	Failure.	Per ct.
1	249	80	32.1	78	31.3	91	36.5	154	53	34.4	49	31.8	52	33.7	95	27	28.4	29	30.5	39	41.0
2	426	113	26.5	115	26.9	198	46.4	241	65	26.9	70	29.0	106	43.9	185	48	25.9	45	24.3	92	49.7
3	402	122	26.4	138	29.8	202	43.7	319	84	26.3	95	29.7	140	43.8	143	38	25.1	43	30.0	62	43.3
4	315	91	28.8	94	29.8	130	41.2	218	64	28.4	64	28.4	90	41.2	97	27	27.8	30	30.9	40	41.2
5	191	53	27.7	61	31.9	77	40.3	150	42	28.0	47	31.3	61	40.6	41	11	26.8	14	34.1	16	39.1
6	189	51	26.9	54	28.5	84	44.4	152	40	26.3	44	28.9	68	41.7	37	11	29.7	10	27.0	16	43.2
7	26	8	30.7	8	30.7	10	38.4	23	6	26.0	7	30.4	10	43.4	3	2	66.6	1	33.3
8	8	1	12.5	3	37.5	4	50.0	6	1	16.6	2	33.3	3	50.0	2	1	50.0
9	2	2	100.0	2	2	100.0
10	3	1	33.3	2	66.6	2	1	50.0	1	50.0	1	1	100.0
12	1	1	100.0	1	1	100.0
	1872	519	27.7	553	29.5	800	42.7	1268	355	27.9	380	29.9	533	42.0	604	164	27.1	173	28.6	267	44.2

* I have altered the form of this table from the original, and calculated the per-centages of success, in order to render the results more clear.

TABLE XII.

Showing the results of adult vaccinations performed in the Wirtemberg Army during five years, terminating in June, 1836.

[Copied from Simon's Report 'Papers,' &c., p. xxxiii.]

	Total.	Ratio of success per 1000 cases vaccinated.		
		Perfect success.	Modified success.	No success.
Vaccination of the Wirtemberg Army in the five years 1831½—5½	14,384	340·2	260·8	411·5

13,681 of the above-mentioned military vaccinations being classified according to the marks of previous vaccination or smallpox, the results were as under :

Degree of success of revaccination.	Of cases with normal cicatrices of vaccination there were 7845, and among these the results per 1000 were—	Of cases with de- fective cicatrices of vaccination there were 3545, and among these the results per 1000 were—	Of cases with no cicatrices of vac- cination or small- pox there were 2025, and among these the results per 1000 were—	Of cases bearing marks of previous smallpox there were 266, and among these the results as per 1000 were—
Perfect . .	310·4	280·7	337·3	319·5
Modified . .	280·5	259·0	191·1	248·1
None . .	409·2	460·4	471·6	432·3

TABLE XII—*continued.*

11,565 of the same number being distributed according to age, the results were as under :

Degree of success of revaccination.	Under 20 years of age there were revaccinated 124 persons, and the results per 1000 were—	Between 20 and 30 years of age there were revaccinated 11,157 persons, and the results per 1000 were—	About 30 years of age there were revaccinated 284 persons, and the results per 1000 were—
Perfect . . .	338·7	285·6	426·1
Modified . . .	322·6	259·2	207·7
None . . .	338·7	455·2	366·2

TABLE XIII.

Showing the number of persons affected with smallpox at different ages, and the fatality (dividing them into vaccinated and unvaccinated), admitted into the Smallpox Hospital in the epidemic season, 1838.

[Gregory, 'Library of Pract. Med.,' vol. i, p. 306.]

	Unvaccinated.		Vaccinated.	
	Admitted.	Died.	Admitted.	Died.
Under 5 years of age	42	20
5 — 9	37	11	5	...
10 — 14	30	8	25	...
15 — 19	104	32	90	6
20 — 24	115	50	106	16
25 — 30	45	23	55	8
30 — 35	12	7	13	1
Above 35 years.	11	6	4	...
Total	396	157 or 39 per cent.	298	31 or 10 per cent.

TABLE XIV.*

Showing the number of persons, protected and unprotected, admitted into Smallpox Hospital, London, from 1836 to 1851 inclusive, with their ages and rate per cent. of mortality at different periods of life.

[Marson, 'Med.-Chir. Trans.,' vol. xxxvi.]

	Unvaccinated.			Vaccinated.		
	Cases.	Deaths.	Per cent. of deaths.	Cases.	Deaths.	Per cent. of deaths.*
Under 5 years	356	181	50	7	2	18.5
5 — 10	334	91	27	56	7	10.7
10 — 15	270	62	23	206	10	4.8
15 — 20	571	154	26	866	49	5.6
20 — 25	669	274	40	1058	93	8.7
25 — 30	270	124	45	526	55	10.4
30 — 40	154	89	57	312	41	13.1
40 — 50	18	13	69	61	10	16.3
50 — 60	8	5		2	1	50.0
60 — 70	2	1	75			
70 — 80	1	1				
80 — 90	1	1				
Total	2654	996	37	3094	268	6.76

* Abstract of two tables. I have added per-centage of fatality of vaccinated cases.

TABLE XV.

Showing the age of 530 persons attacked with smallpox in the Norwich epidemic of 1819. (Cross.)

Under 2 years	Cases.
2—4	„	260
4—6	„	132
6—8	„	85
8—10	„	26
10—15	„	17
15—20	„	5
20—35	„	2
30—40	„	2
							1
							530

TABLE XVa.

Showing the ages of the vaccinated and unvaccinated respectively attacked with smallpox, and treated in the Royal Infirmary at Edinburgh in the epidemic season of 1863.

[Mr. Crossby, 'Med. Times and Gazette,' 1863, vol. ii, p. 44.]

Number admitted.	Age.	Vaccinated.	Not vaccinated.	Doubtful.
7	Under 1 year	0	7	...
26	1 — 5	6	20	...
33	5 — 10	14	19	...
34	10 — 15	18	15	1
66	15 — 20	42	17	7
106	20 — 25	68	29	9
52	25 — 30	37	12	3
6	30 — 35	5	1	...
12	35 — 40	10	2	...
4	40 — 45	3	1	...
4	45 — 50	3	1	...
	Total	206	124	20

TABLE XVI.

Showing the age at which smallpox occurred after vaccination; Württemberg, 1831—1836. (Heim.)

	Normal cicatrices.	Bad cicatrices.	AGE.												Total.
			Under 1 yr.	2 3 4 5	6 7 8 9 10	11 12 13 14 15	16 17 18 19 20	21 22 23 24 25	26 27 28 29 30	31 32 33 34 35					
Varioloid	767	102	13 4 3 6 8	8 9 16 14 12	10 18 35 35 47	39 48 35 47 60	88 34 45 44 39	36 35 24 35 12	12 11 1 30 6	869					
Smallpox	147	39	2 0 1 1 2	1 3 0 3 2	4 3 9 10 15	9 11 8 10 8	6 6 5 9 13	10 6 3 6 5	4 2 5 2 2	186					
Total	914	141	15 4 4 7 10 40	9 12 16 17 14 68	14 21 44 45 62 186	48 59 43 57 68 275	44 40 50 53 52 239	46 41 27 41 17 172	16 13 6 32 8 75	1055					

TABLE XVII.

Showing the number of persons, vaccinated and unvaccinated, admitted with smallpox into the General Hospital at Vienna, specifying their ages and results.

[Constructed from data, pp. 137 and 138 of 'Papers relating to Vaccination.']

Ages in years.	Number of cases.	Vaccinated.		Unvaccinated.	
		Number of cases.	Deaths per 100.	Number of cases.	Deaths per 100.
1 to 10	418	234	14·9	184	40·0
11 „ 20	2634	2228	3·7	406	20·4
21 „ 30	2671	2329	5·6	342	33·6
31 „ 40	406	354	5·9	52	46·2
41 „ 50	84	72	5·5	12	33·3
and upwards.					

TABLE XVIII.

Showing the results of 87 revaccinations of natives of India performed by Assistant-Surgeon Russel, arranging the cases according to the age at which primary vaccination was undergone.

[Abridged from Dr. Duncan Stewart, p. 165.]

Age at which primary vaccination was performed.	Number of persons.	Age when revaccinated.	Successes.
Under 1 year	11	8 to 34 years	2
1 year	23	11 „ 37 „	2
2 years	16	7 „ 43 „	...
3 „	10	9 „ 26 „	...
4 „	6	15 „ 21 „	...
5 „	7	7 „ 32 „	1
6 „	2	22 „ 30 „	...
7 „	5	12 „ 32 „	...
8 „	3	17 „ 32 „	...
9 „	1	13 „	...
10 „	1	17 „	...
11 „	1	15 „	...
19 „	1	27 „	...

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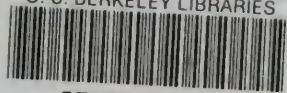
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